TRANSACTIONS

OF THE

AMERICAN CERAMIC SOCIETY

CONTAINING THE PAPERS AND DISCUSSIONS

OF THE

THIRD ANNUAL MEETING

HELD AT

OLD POINT COMFORT, VIRGINIA
FEBRUARY 11-13, 1901

PUBLISHED BY THE SOCIETY
SECRETARY'S OFFICE, COLUMBUS, OHIO
AUGUST 1, 1901

Press of Hann & Adair,
108 North High St.
Columbus

CONTENTS.

PAPERS.

Contents — Continued.

DISCUSSIONS ON ANNOUNCED TOPICS.

MISCELLANEOUS.

OFFICERS.

FOR THE YEAR ENDING FEBRUARY, 1902.

PRESIDENT

CHARLES F. BINNS, - - - - Alfred, N. Y.

VICE PRESIDENT

ERNEST MAYER, - - - - New Brighton, Pa.

SECRETARY

EDWARD ORTON, JR., E. M., - - Columbus, Ohio.

TREASURER

STANLEY G. BURT, - - - Cincinnati, Ohio.

MANAGERS

WILLIAM H. ZIMMER, Ph. D., - - Coburg, Germany.
Term expires 1902

HERBERT A. WHEELER, E. M., - - St. Louis, Mo.
Term expires 1903

ALBERT V. BLEININGER, B. Sc., - Columbus, Ohio.
Term expires 1904

HONORARY MEMBERS.

MRS. MARIA LONGWORTH STORER.
(Mrs. Bellamy Storer)
Cincinnati, Ohio.

FULL MEMBERS.

Binns, Charles F., Alfred, N. Y.	Director, New York State School of Clayworking and Ceramics.
Bleininger, Albert V., B. Sc., Columbus, Ohio	Instructor in Ceramics, Ohio State University.
Burt, Stanley G., Cincinnati, Ohio	Rookwood Pottery Company.
Cannan, William, Jr., Syracuse, N. Y.	Onondaga Pottery Company.
Fickes, Walter M., E. M., Carnegie, Pa.	Rosslyn Brick Company.
Fiske, J. Parker, B. S., Boston, Mass.	Boston Fire Brick Company.
Gates, William D., Chicago, Ill.	American Terra Cotta and Ceramic Company.
Geijsbeek, Samuel, Golden, Colo.	Geijsbeek Pottery Company.
Giessen, Carl, Garfield, Ohio	Brick Mauufacturer.
Gorton, Elmer E., Rome, Ga.	Superintendent Rome Brick Co.
Griffin, Henry R., C. E., Phœnixville, Pa.	
Hottinger, Adolph F., Chicago, Ill.	Northwestern Terra Cotta Co.
Langenbeck, Karl, Zanesville, Ohio	Mosaic Tile Company.
Lovejoy, Ellis, E. M., Union Furnace, Ohio	Columbus Brick and Terra Cotta Company.
Mayer, Ernest, Beaver Falls, Pa.	Mayer Pottery Company.
Ogle, Earl McK., Indianapolis, Ind.	United States Encaustic Tile Wks.
Orton, Edward, Jr., E. M., Columbus, Ohio	Director, Department of Ceramics, Ohio State University.

Pass, James, Syracuse, N. Y.	Onondaga Pottery Company.
Richardson, Willard D., Cleveland, Ohio	Brick Manufacturer.
Ries, Heinrich, Ph. D., Ithaca, N. Y.	Professor of Economic Geology, Cornell University.
Stover, Edwin C., Trenton, N. J.	Trenton Potteries Company.
Walker, Francis W., Beaver Falls, Pa.	Beaver Falls Art Tile Company.
Wheeler, Herbert A., E. M., St. Louis, Mo.	Elizabeth Lead Company.
Zimmer, William H., Ph. D., Coburg, Germany	Redaction Sprechsaal.

ASSOCIATE MEMBERS.

Alrich, John C., Pittsburgh, Pa.	Star Encaustic Tile Company.
Bloomfield, Charles A., 58 Quincy St., Brooklyn, N. Y.	Dealer in Clays.
Bowman, William J. J., C. E., Trenton, N. J.	Trenton Fireclay and Porcelain Company.
Britton, Dr. Charles P., Trenton, N. J.	Star Porcelain Works.
Burgess, William, M. Sc., Trenton, N. J.	International Pottery Company.
Campbell, Augustine R., Tottenville, Staten Island, N. Y.	Atlantic Terra Cotta Company.
Carter, Owen, "Wykeham," Poole, England	Carter Encaustic Tile Works.
Diedrick, William H., East Liverpool, Ohio	Sevres China Company.
Edwards, Clarence J., B. Sc., Newberg, Oregon	Newberg Press Brick Company.
Evatt, Frank G., Corning, N. Y.	Traveling Salesman, for M. E. Gregory.
Ferguson, Silas M., Wellsville, Ohio	United States Pottery Company.
Forst, Arthur D., B. S., Morrisville, Pa.	Robertson Art Tile Company.
Franzheim, Charles, Wheeling, W. Va.	Wheeling Pottery Company.
Frerichs, William D., Tottenville, Staten Island, N. Y.	Atlantic Terra Cotta Company.
Gates, William P., Terra Cotta, Ill.	American Terra Cotta and Ceramic Company.
*Golding, William, M. E., Trenton, N. J.	The Golding Sons Company.
Gray, Thomas, East Boston, Mass.	New England Pottery Company.

---

* Deceased.

Harris, Charles T., New York, N. Y.	Celadon Roofing Tile Company.
Hasburg, John W., 588 Fullerton Ave., Chicago, Ill.	Dealer in Ceramic Materials.
Hasslacher, Jacob, New York, N. Y.	Roessler and Hasslacher Chemical Company.
Hensel, Otto, Chicago Heights, Ill.	Ludowici Roofing Tile Company.
Hice, Richard R., Beaver, Pa.	Fallston Fire-clay Company.
Hunt, William H., Cleveland, Ohio	Cleveland Hydraulic Pressed Brick Company.
Junge, William H., Chicago, Ill.	Northwestern Terra Cotta Co.
Koch, Charles Frederich, Covington, Ky.	Cambridge Tile Manufacturing Co.
Land, Dr. C. H., Detroit, Mich.	Manufacturer, Dental Furnaces.
Locke, Frederick M., Victor, N. Y.	Manufacturer Porcelain Insulators
Maddock, Archibald M., Trenton, N. J.	Thomas Maddock & Sons.
Maddock, John, Trenton, N. J.	John Maddock & Sons.
McRay, John T., Brazil, Indiana	McRay Clay Company.
Middleton, Jefferson, Washington, D. C.	Statistician, United States Geological Survey.
Moses, James, Trenton, N. J.	Mercer Pottery Company.
Parker, Lemon, St. Louis, Mo.	Parker-Russell Mining and Manufacturing Company.
Plant, Alfred, Keyport, N. J.	Eagle Tile Company.
Pope, Charles A., Trenton, N. J.	Trenton Fire Brick Company.
Powell, William H., New York, N. Y.	Excelsior Terra Cotta Company.
Purdy, Ross C., Zanesville, Ohio	Roseville Pottery Company.

Ramsay, Andrew, Mt. Savage, Maryland	Mt. Savage Enameled Brick Co.
Randall, Theodore A., Indianapolis, Ind.	Editor "Clay-worker."
Rix, Wilton Parker, 34 Liverpool Road, Newcastle, Staffordshire, England	
Russell, Joseph, 1302 Queen St., East, Toronto, Canada	Brick Manufacturer.
Shaw, Frances D., 927 Edgewood Ave., Trenton, N. J.	
Skeele, H. B., Chicago, Ill.	Ludowici Roofing Tile Company.
Smith, Amedee M., Portland, Oregon	Western Clay Manufacturing Co.
Stanbery, George A., Zanesville, Ohio	American Encaustic Tile Company, Ltd.
Sweet, George A., F. G. S., Melbourne, Australia	
Thomas, David C., Wellsville, Ohio	United States Pottery Company.
Titcomb, W. G., Providence, R. I.	Brick Manufacturer.
Townsend, Everett, Morrisville, Pa.	Robertson Art Tile Company.
Trommer, Carl E., New York, N. Y.	Roessler and Hasslacher Chemical Company.
Voelcker, Gustav, Lancaster, N. Y.	Lancaster Face Brick Company.
Walker, John A., Jersey City, N. J.	Joseph Dixon Crucible Company.
Whitehead, C. Louis, Trenton, N. J.	Providential Tile Works.
Williams, Ira A., B. S., Ames, Iowa	Ceramic Department, Iowa State University.
Yates, Alfred, Johnsonburg, Pa.	Johnsonburg Vitrified Brick Co.

RULES.

OBJECTS.

The objects of the American Ceramic Society are to promote the arts and sciences connected with Ceramics by means of meetings for social intercourse, for the reading and discussion of professional papers, and for the publication of professional literature.

MEMBERSHIP.

The Society shall consist of Honorary Members, Members and Associates.

Honorary Members must be persons of acknowledged professional eminence, and shall not exceed five in number.

Members shall be persons competent to fill responsible positions in Ceramics and have suitable qualifications.

Associates shall include persons interested in Ceramics and the allied arts.

Honorary Members shall be proposed by at least five Members, approved by the Council, and receive at least 90 per cent. of the votes cast by letter ballot at the annual meeting.

Members and Associates shall be proposed by at least three Members or Associates, approved by the Council, and receive at least 75 per cent. of the votes cast by letter ballot. A candidate for admission must make application on a form prepared by the Council which shall contain a written statement of his age, professional experience, and that he will, if elected, conform to the laws, rules, and requirements of the Society.

All Honorary Members, Members, and Associates shall be equally entitled to the privileges of membership, except that only Members shall be entitled to hold office and to vote. Applicants for a change in grade of membership shall conform to the requirements of a new applicant.

Any person can be stricken from the membership of the Society on the request of five or more Members, on the recommendation of a majority of the Council, if he fails to resign on the advice of the Council. Such person, however, shall first be notified of the charges against him, and be given a reasonable time to appear before the Council, or present a written defense, before final action is taken by the Council.

DUES.

Honorary Members shall be exempt from all dues.

The initiation fee of Members shall be ten dollars, and of Associates five dollars, which if not paid within six months after election, will render the election void.

The annual dues for Members will be fixed by the Council, but shall not exceed five dollars per year.

The annual dues for Associates will be fixed by the Council, but shall not exceed four dollars per year.

Any Member or Associate in arrears for over one year may be suspended from membership by the Council until such arrears are paid.

OFFICERS.

The affairs of the Society shall be managed by a Council, consisting of a President, Vice President, Secretary, Treasurer, and three Managers, who shall be elected from the members at the annual meeting, and hold office until the adjournment of the meeting at which their successors are elected.

The President, Vice President, Secretary and Treasurer shall be elected for one year, and the Managers for three years; and no President, Vice President, or Manager shall be eligible for immediate re-election to the same office.

The duties of all officers shall be such as usually appertain to their offices, or may be delegated to them by the Council or the Society; and the Council may at its discretion require bonds to be furnished by the Treasurer.

Vacancies in any office shall be filled by appointment by the Council, but the new incumbent shall not thereby be rendered ineligible to re-election at the next annual meeting to the same office. On the failure of any officer to execute his duties within a reasonable time, the Council, after duly warning such officer, may declare the office vacant, and appoint a new incumbent.

A majority of the Council shall constitute a quorum; but the Council shall be permitted to carry on such business as it may desire by letter.

ELECTIONS.

At the annual meeting, a nominating committee of five Members, not officers of the Society, shall be appointed, and this committee shall send the names of nominees to the Secretary at least 60 days before the annual meeting, who shall immediately forward the same to the Members. Any other five members may also present the names of any candidates to the Secretary, provided it is done at least 30 days before the annual meeting. The names of all candidates, provided their assent has been obtained, shall be placed on the ballot without distinction as to nomination by the regular or an independent nominating committee, which shall be mailed to every member, not in arrears, at least 20 days before the annual meeting.

The ballot shall be inclosed in an inner blank envelope, and the outer envelope shall be endorsed by the voter, and mailed to the Secretary for collection. The blank envelopes shall be opened by three Scrutineers appointed by the Chair at the annual meeting, who will report the result of the election. A plurality of the votes cast shall elect.

MEETINGS.

The annual meetings shall take place at such time and place as the Council may decide, at which time reports shall be made by the Council, Treasurer, and Scrutineers of election, and the accounts of the Treasurer audited by a committee of three appointed by the Chair.

Other meetings may be held at such times and places during the year as the Council may decide, but at least 20 days' notice shall be given of such meetings.

Seven Members shall constitute a quorum at any regular meeting, and a majority shall rule, except where otherwise specified.

The order of business at the annual meeting shall be.—

1. Reading of Minutes of last meeting.
2. Reports of the Council and Treasurer of the Society.
3. Announcement of Election of Members.
4. Announcement of Election of Officers.
5. Appointment of Nominating Committee.
6. Old Business.
7. New Business.
8. Reading of Papers.

PUBLICATIONS.

The Council shall act as a Publication Committee, and decide as to what to publish. The publications of the Society shall be sent to all Members and Associates not in arrears. The Secretary will furnish each author with reprints of his papers at cost price, provided due notice is given that reprints are desired.

The Society is not, as a body, responsible for the statements or opinions expressed in its publications, papers or discussions.

PARLIAMENTARY STANDARD.

Roberts' "Rules of Order" shall be the parliamentary standard on all points not covered by these rules.

AMENDMENTS.

These rules may be amended at any regular meeting by a two-thirds vote of a letter ballot at the subsequent annual meeting provided a written notice of such proposed change is sent to each Member at least 30 days before said annual meeting. Said proposed amendments shall be printed on the ballot for officers and counted by the same Scrutineers.

PREFACE.

The following pages contain the papers and discussions of the Third Annual Meeting, held at Old Point Comfort, Virginia, February 11th, 12th and 13th, 1901.

This volume comprises the record of the society's most profitable years work. That of last year contained the Manual of Ceramic Calculation, of over eighty pages, and assumed fictitious size on that account. The contributions of the present year, while not more numerous, are longer, and, it is hoped, more thorough and of a higher grade technically than the earlier work of the society.

Although it is not the policy of the society to publish a record of its business meetings, this custom has been over-ridden in one instance on page 227 of this volume. The remarks of President Binns on the election of the first full members from the list of the associates, seemed not only to be peculiarly impressive and felicitous, but also to define in words, a principle which has so imbued the members of the society as to have the force of an unwritten law. For these reasons, it is herein reproduced, and it is hoped that it may recall a memorable scene to those who witnessed it, and inspire in those who did not, a fuller realization of the opportunities for individual growth and development which the society affords.

The growth of the society has continued most rapid. In spite of the fact of the loss of six names from its rolls, the present list shows the flattering increase of over forty per cent., which is certainly as large a proportion as any organization can assimilate and imbue with its own principles and ideals.

For the first time in our short career, we are called upon to chronicle the loss of one of our members by death. The memorial to MR. WILLIAM GOLDING appears on page 225.

The previous publications of this society are comprised in Volumes I and II, and The Manual of Ceramic Calculation. The latter is included in Volume II, or can be purchased separately at the price of $1.00. The price of Volumes I and II is $4.00 each to persons not members of the society, while members can obtain extra copies at 50 cents and $2.00 respectively until further notice.

Volume III will be sent promptly on publication to all members not in arrears; extra copies will be sold to members at the price of $1.00 per copy, and $4.00 to persons not members.

Please address all correspondence for the purchase of publications to STANLEY G. BURT, Treasurer, Rookwood Pottery, Cincinnati, Ohio.

EDWARD ORTON, JR.,
Secretary.

COLUMBUS, O., August 1, 1901.

THE FINENESS OF FLINT IN ITS RELATION TO THE MANUFACTURE OF POTTERY BODIES.

BY

STANLEY G. BURT.

The word Flint, originally of a limited meaning, is now used to cover all types of silica furnished to the potter. In nature, two main varieties occur, the crystalline and the amorphous, either of which may analyze 99 per cent. or over pure SiO_2. Of the crystalline, two kinds are regularly sold, the rock and sand flints, both in the main from the same source. The rock, we to-day mine and grind; the sand, nature, in decomposing the rock, mined for us and partially ground. Flint pebbles, the true original flint, found largely on the sea-shore, especially in France, gives us the amorphous variety, sold by dealers as French flint. Silica is also found of apparently an intermediate character, between the crystalline and the amorphous.

The question now arises, can any one of these types be substituted for the other? It seems incredible, that so important a point has not been settled long ago, but to-day one potter will tell you that rock goes a great deal farther, that is, you need less of it; while another is perfectly satisfied with sand. It is my purpose to give in this paper, the results of a few experiments I have lately carried out, endeavoring to throw a little light on this question. First, however, I purpose to give a brief review of the subject.

In the use of flint in bodies and glazes, there are many points to be considered. Here, however, I wish to merely consider flint in its bearing on the question of crazing. To have a perfect glaze we must have a uniform co-efficient of expansion and contraction, for glaze and body. Expansion and contraction should in no way be confused with shrinkage. A clay body when fired loses water and shrinks. Increased heat gradually fuses the clay, and the body continues to shrink. That is, the clay piece undergoes a permanent decrease of size. This is the shrinkage of the body, and *per se* has nothing to do with its co-efficient of expansion and contraction. It is true that increased heat gives increased shrinkage, and at the same time a constantly changing co-efficient of expansion, but these are are separate and distinct phenomena.

I have often heard it stated by potters, that a vitrified body would craze where a porous one would not, the physical condition of vitrification, being in their minds the cause of the craze. The fact that the vitrified body would craze, might often be true, but this result would be due, *first,* to the fact that from a porous body, the glaze could absorb much more silica, and *second*, to the fact, that in a vitrified body, the silica has passed from the free crystalline state, to the combined amorphous condition, and would not be due, as they would have us believe, to the fact that a porous body permitted the glaze to enter the pores, hold better and not craze. How does an enamel get into the pores of iron?

If we take a piece of pottery in our hands, the heat from the hand immediately causes the clay body and glaze on it to expand. The amount of expansion for one degree of heat, represents the co-efficient of expansion and of contraction on cooling. Each separate ingredient, entering into the composition of the glaze or body, has its own individual co-efficient, which either raises or lowers the co-efficient of the mixture.

In the body, pure kaolin has a low co-efficient, that of spar is also low, but flint has a high one; so Seger gave us the law "to increase the co-efficient of the body, add flint." In a glaze, the case is reversed, lead and most of the bases, have a high co-efficient, compared to which the flint is low; so Seger says, "to decrease the co-efficient of the glaze, add flint." In further explanation of these two laws, we now know, that the crystalline, free, flint in the body, has a higher co-efficient than the amorphous combined flint, in the glaze.

With these two laws, how plain is the difference between the composition of china and porcelain! Both start with pure kaolin for the body, with its low co-efficient; with porcelain, we wish to vitrify this, so we must add spar, also of low co-efficient, and can add but little flint, to raise the total co-efficient. We now have to balance up by adding a great deal of flint to the glaze, and thus bringing the glaze co-efficient down. With china we wish to use a softer glaze containing some lead, and consequently having a high co-efficient; so here we give up the vitrification of the body and add to it all the flint it will carry, to bring its co-efficient up to the lead glaze.

Before leaving the co-efficient question, let me give another example. In making dishes for boiling acids, not only must we aim for a body proof against the action of acids, but we must also provide a body which can stand sudden changes from heat to cold. Obviously a body with high co-efficient will not do; so we leave out just as much flint as possible, replacing it with the low co-efficient aluminum silicate, that is, kaolin.

Before passing on to Seger's next law about flint, I wish to explain, that when I speak of pure kaolin, having a low co-efficient, by pure, I mean at least 95 per cent aluminum silicate. We call any short clay, which burns pure white and stands a high fire, a kaolin, but it would be a mistake to say that a kaolin, showing say 40 per cent. free silica, has a low co-efficient. The rational analysis must always be kept in mind. Any one, for instance, who takes Seger's rule, substitutes a ball clay for a kaolin, to increase the co-efficient, and replaces a kaolin showing 40 per cent free silica, with a ball clay showing but 10, will probably come to grief.

Seger's third rule about flint is, "the finer the flint, the greater the increase in the co-efficient of the body." This rule acts with greater force than either of the other two, that is, a slight variation in fineness will have more effect than a slight increase of flint, in body or glaze. I have seen it stated that one-third less flint very fine, served as well as the full charge not fine. The importance of the fineness of the flint cannot be over-estimated. Unless we have a fine grain, there can be no true separation of the clay particles, we have lumps of flint and lumps of clay, each with its own co-efficient. It is only with the complete mix obtained from fine grains that we get the benefit of the high flint co-efficient. Furthermore, a glaze when completely fused takes up flint from the body; now not only does a glaze made with fine flint fuse much more readily, but the finer the flint in the body, the more of it will be taken up by the glaze.

In considering now, why it was that one could use this flint and not that, and vice versa, there seemed but two explanations: *First*, that it all rested upon the question of fines. This I believe the true explanation. If I am using, say 30 per cent of rock flint, all of which passes a No. 14 lawn and substitute sand flint, which leaves say an 8 per cent. residue on the lawn, even though I allow for this loss, the sand flint which passes the No. 14 lawn, would not be as fine as the rock and craze might result. The same would be true reversing the flints. *Second*, that it rests on the difference in coefficient between crystalline and amorphous flint. How would it be, if, using the crystalline rock, I substituted for it the amorphous French flint. We know that the co-efficient of the amorphous silica is not as high as that of the crystalline. Therefore, would a body using the rock flint and exactly suited to a glaze, show craze if we changed to a French flint of the same fineness of grain? This might be possible, and might also offer an explanation, of why an overfired buscuit piece should craze, for we have in it our free flint combined and no longer crystalline. The vitrified body, thus produced, allowing of less absorption of silica by the glaze, is an additional factor.

Coming now to the experiments, the points to be shown are, *first*, with a uniform fineness of grain, is there any difference between rock and sand flint, in a body or glaze, and *second*, with the same fineness, can French flint be directly substituted for either of these? It must always be borne in mind, that fortunately for us, glazes have a certain elasticity, allowing what I call a margin of safety. Between craze on the one hand and peeling on the other, there is considerable range, within which, the glaze is all right. On this subject of the margin of safety, I hope at some future date, to have a paper for the Society, showing whether it is greater with an increased body co-efficient, or a decreased glaze co-efficient. What I wish to call your attention to here, is that unless there is a pronounced difference between the co-efficient of the amorphous and the crystalline silica in the body, a variation might not be noticeable.

For testing the various flints, I got samples from different dealers and first examined their fineness of grain. I passed each through a No. 14 lawn, then through a No. 16 lawn and found an astonishing difference, some showed as high as 15.8 per cent. residue on the No. 16 lawn, while others showed but 0.5. per cent. As my time was too limited to arrange an apparatus by which I could get an absolute uniformity in size of grain, I decided to grind all, so as to pass the No. 16 lawn.

Two other factors should be considered here, *first*, wet or dry grinding, and *second*, calcining. While I am not a microscopist and am open to conviction of my error, my firm belief is that in this connection it is immaterial whether we grind wet or dry; it all depends upon the result obtained, that is, the fineness of grain of our product, and that it is the fineness and not the shape of the particle, which affects the co-efficient.

As for calcining, theoretically I can see no ground for its causing any change in the flint, still it is an interesting point, which a future series of trials will have to determine. In the samples I experimented with, the rock and French flints were calcined, the sand was not.

Five samples were selected. Number one and number two were American rock flints, number three a French flint and numbers four and five, sand flint. The last sample was washed from a fine white kaolin, and was furnished without grinding. On the lawns, these gave the following results:

Number of the Sample	Derivation of the Material	Residue on a No. 14 lawn	Residue on a No. 16 lawn	Total Residue	Designation of bodies derived from these flints	Designation of glazes derived from these flints
I	Amer. Rock	3.7	8.2	11.90	1 A	1 B
II	" "	0.2	0.3	0.50	2 A	2 B
III	French	4.9	5.9	10.80	3 A	3 B
IV	Sand	1.7	1.5	4.20	4 A	4 B
V	"	0.30	7.3	7.60	5 A	5 B

I first made cones of these five and fired to cone 9. Number 1 had become quite firm, would not mark; number 2, number 3, and number 4 marked easily; number 5 would hardly hold together. I now fired other cones of these same flints to cone 25. Results showed 1, 3 and 5 quite hard; 2 and 4 still soft.

My next step, was to make five bodies, with a fixed formula, using each of the above flints in turn. These I called 1 A, 2 A, etc. Of these, cones were made and fired to cone 9 and cone 25. No marked difference could be noted in the bodies, at either heat. All vitrified at cone 9, none had fused at cone 25. I then cast pieces of pottery from each of these bodies in the same mold, and fired all in the same sagger.

Five glazes were next made with the samples, using a fixed formula, known to be nearly correct for the body formula used, but one which would in time show craze. These were numbered 1B, 2B, etc. Of these cones were made, and the comparative fusibility determined. Glazes using flints 1, 2, 3 and 4 were very uniform, practically identical, those, which were originally the finest ground, are a shade the softest. Glaze using flint No. 5 is plainly a little harder. These five glazes were tried on uniform pieces of the No. 1 A body, precaution being taken at each step, to give a uniform test.

The glazes came from the kiln free from craze and remained so for two days, then 1B began to craze, this was followed by 3B and 4B and then later by 5B, 2B remaining free. So one Rock flint, both sand flints, and the French flint craze, while the other Rock flint remained free. The kind of flint certainly seemed of no consequence here.

The pieces made from the various bodies were all dipped in glaze 1B. It was a week before any of these showed craze, then 1, 3 and 4B showed a trace, while 2 and 5 were free. So of the glazes, the first to craze was a Rock flint, but the only one that, in time did not craze, was also a Rock flint. Between sand number 4 and French, there is but little choice, while sand number 5 is a little better than these two. Of the bodies, 1, 3 and 4A, show a very slight craze, of which 3A is a little the worse, not enough, however, to warrant saying positively, that the amorphous French showed a noticeably different co-efficient from the others. No. 2A and 5A are free from craze. Drawing conclusions from the experiments, we find, *First*, if there is any difference between the action of French and the others, it must be very slight. *Second*, that with uniform fineness of grain, just as good results can be obtained, in using any of the flints, whether it be Rock, sand or French, in the glaze and as far as can be judged, from these trials, in the body also. *Third*, and finally, most important of all, is the fineness of grain. Number 1 American Rock, the sample containing originally the coarsest grain, showed most craze, while number 2 American Rock, the finest grained sample, shows the least.

DISCUSSION

Mr. Edward C. Stover: I would like to ask whether the testing was done with an ordinary hand lawn, or whether Mr. Burt had any special apparatus for testing the fineness?

Mr. Burt: Just through a hand lawn.

Mr. Stover: I would like to ask what number of meshes to the inch corresponds to your number sixteen?

Mr. Burt: 160 to the lineal inch, or 160 x 160 per square inch.

The Chair: Not exactly that, but nominally that.

Mr. Stover: You did not take what passed the 140 and was rejected by the 160 between the two?

Mr. Burt: No.

Mr. Stover: You took only what went through both?

Mr. Burt: Yes, I had no opportunity to arrange floating apparatus to float off the finest sizes. I aimed to get results in which I would have sufficient confidence to go ahead and use them in any regular work. The point which I make in my paper is that if you compare a flint which passes a number fourteen lawn completely, with another which leaves large per cent residue, the former is going to be much finer flint, not only in the grains which refuse to pass the lawn, but also in those which do pass it. Its grains will average finer that those of the flint has a coarse residue.

Mr. Stover: A gentleman in Trenton once brought up this same question, and I believe there was a series of experiments made by the Eureka Spar Company where the same spar was ground the ordinary length of time, something like four hours, then double that time, than three and four times the usual period. It was then lawned off and they found the difference in fineness did not compensate for the difference in time of grinding. With double grinding, there was little difference in the fineness of the product, measured by passage through the lawns.

Mr. Burt: Mr. President, there is another point which I do not touch upon in my paper. Some people claim you can grind a flint too fine, but I don't think we will ever run any danger from this source.

The Chair: Dr. Zimmer has a paper along this same general line, and unless someone now desires to discuss Mr. Burt's paper further, it would be well to hear Dr. Zimmer's paper before dropping this subject.

HYDRATED SILICIC ACID IN KAOLIN AND ITS EFFECT ON POTTERY BODIES.

BY

WILLIAM H. ZIMMER, PH. D.

The prototype of all our various clays is the mineral kaolinite, the chemical formula of which, Al_2O_3, $2\ SiO_2$, H_2O, is generally adopted as the formula of clay substance; in fact, up to date, it is the only one considered in ceramic calculations. Yet there seems to be here, in our own country, quite a number of hydrous silicates of alumina, which, although classified in a commercial way as Kaolins, do not correspond exactly to the formula of kaolinite. We find allusions in the Indiana Geological Survey reports, to a mineral Indianaite, which contains Al_2O_3, $2\ SiO_2$, $4\ H_2O$, the latter being combined. Cook, in his reports on New Jersey clays, mentions also the presence of free hydrated silica in quite a number of instances.

I venture to say that these cases are not generally known to those interested in the manufacturing of ceramics, and I must confess that I myself was not posted about these facts, when in 1897 I received for testing samples of some southern clays, where the physical properties and chemical analysis seemingly disagreed. The sample was too small for a thorough investigation, and as my time was pretty well occupied by the regular factory work, I had no chance to take the matter up again, until last summer I received another very pure, white sample from about the same locality.

The clay was rather hard and had to be ground in order to mix well with water. It left hardly any residue on a 130 mesh sieve. The linear contraction from the moist, damp state, fit to be worked in the clay-shop, to perfectly air-dry, was 13.54 per cent.; the shrinkage from air-dry to cone 7 was

15.62 per cent., making a total of 29.16 per cent. and surpassing that of all our well-known pottery clays. The clay burnt glass hard at that temperature, and was non-absorbent. Its color was still a splendid white, and, even when glazed, it showed a better color than the very best English china clays. Plasticity and strength were also remarkably high. Introduced into china-bodies,—true porcelain as well as bone-china,—the result was a lowering of the temperature of fusion, giving translucent bodies at a heat decidedly below the one necessary to cause the same degree of translucency where one of the standard kaolins was used.

Still more interesting was the result of the chemical investigation. The rational analysis showed only a residue of 0.05 per cent. not decomposed by H_2SO_4, and according to all rules and regulations of our general ceramic creed, I should have been entitled to take the sample for the ideal pure kaolin of the formula Al_2O_3, 2 SiO_2, 2 H_2O, corresponding to 39.54 per cent. Al_2O_3, 46.51 per cent. SiO_2, 13 95 H_2O. To my greatest surprise the analysis gave the following result:

SiO_2	57.00
Al_2O_3	24.85
Fe_2O_3	0.25
CaO	0.05
H_2O	17.81
	99.96

It is evident that under the old supposition that I would have to deal with a kaolin of the standard formula Al_2O_3, 2 SiO_2, 2 H_2O, the result of the chemical analysis did not agree with the rational analysis. Finally I came to the conclusion that there must be present free hydrated silicic acid, although I did not know of any similar cases, until Professor Orton told me that he had had some experience on the same line, and kindly encouraged me to go ahead with my investigation. I developed for this kaolin sample the formula:

$$Al_2O_3,\ 2\ SiO_2,\ 2\ H_2O + 2\ H_2SiO_3 \text{ or}$$
$$Al_2O_3,\ 4\ SiO_2,\ 4\ H_2O.$$

The theoretical composition of such a substance would be

SiO_2	57.83
Al_2O_3	24.84
H_2O	17.33

This composition is almost identically the same as given by the chemical analysis. If we are justified in ascribing to his kaolin such a constitution, the result of the rational analysis is explained in a very satisfactory manner, as naturally free hydrated silicic acid will be easily dissolved by by the repeated treatment with NaOH. It will also explain the behavior of the clay in the kiln and in china bodies, as it is to be expected that in presence of fluxes, free silicic acid will become a reagent more active than flint, as soon as the chemically combined water begins to be expelled.

The question now arose as to whether all such kaolins would show free hydrous silicic acid in certain proportions, corresponding to this new formula, or since the relation between the molecules of the clay substance proper and free hydrous silicic acid would be rather loose, whether this latter would not be more or less washed out during the process of decomposition which undoubtedly originated those kaolins. I succeeded in obtaining small samples of two other clay beds, which gave me the following results:

Sample II was also very white and hard as if indurated by hydrous silicic acid. The powdered substance mixed with water would pass through a 150m. sieve without leaving any residue and the rational analysis indicated only 0.10 per cent. mineral detritus.

The pulverized substance was dried 48 hours over H_2SO_4 in a dessicator to apparently constant weight, but lost in drying oven heated to 120° Cels. 2.78 per cent. water, which loss I would attribute to water combined with the silicic acid. After 4 hours drying, the weight was constant. The chemical analysis gave the following figures:

SiO_2	50.00
Al_2O_3	35.28
Fe_2O_3	0.15
CaO	0.57
Loss by ignition	
Chem.comb. H_2O	14.28
	100.28

This kaolin contained undoubtedly a rather small percentage of free silicic acid and as the 2.78 per cent. water lost in drying oven at 120 degrees Cels. might have included some moisture retained even during the process of drying over conc H_2SO_4. I tried to make a direct determination of the free silicic acid by treating the substance with a solution of caustic soda. All free silicic acid ought to go into solution, but it is very difficult to properly wash the clay on the filter unless you filter under pressure. Even the repeated settling of the substance and decanting of the supernatant liquid is a very slow process. The silicic acid was precipitated by HCl and treated as usual; but as caustic soda might have attacked to a certain extent the clay substance proper, I determined also the small amount of alumina in solution and figured from the alumina the combined silica that was to be subtracted from the total amount of silicic acid dissolved by caustic soda, in order to determine the real amount of the free acid. The result gave me 6.05 per cent. free silicic acid, a percentage which I consider rather too low, on account of imperfect washing out, but which strengthens the proof of the presence of free hydrous silicic acid although the figure might be not quite exact.

Sample III had again the same physical properties as the previous sample, only it was still harder, flinty I might say, and needed grinding in order to mix with water to a plastic body.

The substance was dried at first in desiccator, and lost afterwards in drying oven at 120 degrees Cels. 9.09 per cent. water. Heated in platinum crucible to red heat it lost 7.56 per cent. more. The complete analysis of the air-dry substance gave the following result:

SiO_2	68.63
Al_2O_3	15.00
Fe_2O_3	0.60
CaO MgO	trace
H_2O	16.65
	100.88

The substance dried at 120 degrees Cels. contained

SiO_2	75.50
Al_2O_3	16.50
Fe_2O_3	0.66
H_2O	7.56
	100.22

By treatment with NaOH, I extracted 31.41 per cent. SiO_2 and the same time by determining the Al_2O_3, I found that 3.97 per cent. clay substance proper had been decomposed. Consequently I had to subtract 1.84 per cent. SiO_2 from total above. This would leave 29.57 per cent. Silica, which had been present in the free hydrous state. Figuring from the loss of 9.09 H_2O in drying oven, there should have been 30.29 per cent. SiO_2. Both figures are undoubtedly rather too low. The rational analysis of the substance dried at 120° gave me a residue 1.26 per cent., containing 0.91 per cent. Silica, while 0.35 per cent. not volatilized by hydrofluoric acid, consisted mostly of Fe_2O_3.

The presence of free hydrated silicic acid was proven in all three samples beyond any doubt, yet I must say that it was impossible to establish one formula covering all three instances unless we would write the formula :

$$Al_2O_3, 2\,SiO_2, 2\,H_2O + x, H_2SiO_3,$$

where x denotes the varying amount of the free hydrated silicic acid. The variation is considerable, but as there are a good many possibilities of having free hydrated silicic acid removed from the remaining kaolin, such variation is not at all surprising and would even not contradict the theory, that the original mineral in all three cases has been the same. Whatever this mineral has been, I do not venture to say, without a more thorough knowledge of the geological condition of those sections of our country. Perhaps these clays originated by decomposition of volcanic tuffs or other plutonic rock on the bottom of the deep sea, which receded in more recent periods and finally left the claybed on dry land. At any rate the presence of such large amounts of free silicic acid is an almost direct proof against the theory of Collins (see Ries. Transact. A. C. S., vol. II, page 94,) that kaolinite is derived from feldspar by fluoric vapors, which would have probably increased the percentage of alumina, but would hardly have left any perceptible quantity of hydrous silicic acid.

Those three clay samples with free hydrated silicic acid had, however, properties so characteristic that I went further to define more exactly the influence of free hydrated silicic acid on well known clays.

I selected for this purpose Zettlitz kaolin, the analysis of which is given by Seger, Ges. Schriften, page 182, as follows:

SiO_2	46.87
Al_2O_3	38.56
Fe_2O_3	0.83
CaO MgO	trace
Alkalies	1.06
Loss by ignition	12.73
	100.05

and Tennessee ball clay from L. Mandle, which, according to Mr. S. Geysbeek's rational analysis, consists of 86.20 per cent. clay substance, 2.70 feldspathic minerals and 11.10 per cent. quartz. Both were treated alike. To one pound dry clay, mixed with water to the consistency of a good slip, I added 16 oz. of the ordinary commercial solution of silicate of sodium; the slip was acidulated with HCl and coagulated at once to particles not unsimilar to those of the natural clay sample with free acid when immersed in water. By repeated washing, sieving and pressing, the surplus of muriatic acid and also the chloride of sodium was removed as completely as possible.

On pressing into plaster-paris moulds of 4 inches diameter, in the form of small round disks, these two clay bodies with free hydrated silicic acid behaved in a manner analogous to the three sample clays. They had been indurated by the hydrous silicic acid, and the little disks made from the prepared Zettlitz kaolin stood five times the weight that was sufficient to break one made out of the pure kaolin without any of the hydrated silicic acid. An addition of flint corresponding to the same amount of SiO_2 would have weakened the strength. In the same way, an addition of flint would reduce shrinkage. Pure Tennessee ball clay will shrink in drying 4.90 per cent.; this percentage will remain almost constant when carrying only small amounts of flint, but will finally drop to 2.80 per cent. in a body of 25 parts Tennessee ball and 75 per cent. Flint and about 1 per cent. in a body with 87.5 per cent. flint.

The following table contrasts the relative shrinkage of the clays with and without the H_2SiO_3 as well as one of the natural samples:

	Pure Zettlitz	Prepared Zettlitz	Pure Tennessee	Prepared Tennessee	Sample No. 1
Shrinkage in drying	1.56	12.50	4.90	16.67	13.54
Shrinkage in fire (cone 8).	6.25	7.50	6.90	13.28	15.62
Total shrinkage ...	7.81	20.00	11.80	29.95	29.16

At the same time, these prepared clay trials containing free hydrated silicic acid showed, after firing, a decidedly better color; a fact which could not be attributed to a mere addition of a white substance like silica, but which probably we will have to explain by a chemical process, in as much as free amorphous silicic acid is more likely to bind Fe_2O_3 to a more or less colorless silicate even in oxidizing fire than the crystalline flint.

I have here (holding up samples) two china bodies, both consisting of Zettlitz kaolin, spar and silica in exactly the same proportion, only one contained originally part of its silica as free hydrated silicic acid. Both were fired at cone 8 in the very same sagger. The fire was nearly all the time oxydizing, yet there is quite a contrast in the color of both and the greater translucency of the one with better color indicates at the same time that free silicic acid is a more active reagent in the heat than quartz, and has a tendency to lower the vitrification point of china bodies. (The samples were here passed around the audience and exhibited a very marked difference in translucency.)

A subsequent experiment led me to change my opinion in this matter, to a certain extent.

I precipitated silicic acid out of a solution of silicate of soda by muriatic acid, and washed for two days until a test with nitrate of silver indicated that the last traces of acid and chloride had been removed. The residue was allowed to dry without any artificial means, and, tested immediately before being used, it yet contained 43 per cent. H_2O.

The spar applied in the following experiment was from F. H. Silkmann, New York, and contained according to the analysis by Sharpless, Boston:

SiO_2	63.50
Al_2O_3	19.68
Fe_2O_3	0.12
Na_2O	4.88
H_2O	11.27
Loss by ignition	0.30
CaO, MgO	traces
	99.75

The kaolin was the same Zettlitz kaolin, used in previous tests. A body, containing 40 per cent. kaolin, 40 per cent. spar and 20 per cent. silica, corresponding to the following actual weights:

Zettlitz kaolin	40.00	
Spar	40.00	
amorph., precipit. Silicic Acid	35.08	(less 43% moisture $= 20.00\ SiO_2$)

was very short and could hardly be worked, while in the previous experiments and in all instances where the natural clay containing free hydrated silicic acid was used, the bodies were very plastic after being properly ground.

A second body was made according to the same formula, and out of the same materials, except that the amorphous silica was replaced by a dry ground flint containing 99 per cent. SiO_2 and 0.51 per cent. moisture. The actual weights were Zettlitz kaolin 40, spar 40, dry ground flint 20.1 part. Although it was a short body, this mixture was very much easier worked than the mixture with amorphous silica. Both were fired in the very same saggar to a temperature between cones 7 and 8. Mixture A with amorphous silica was nicely translucent, with a slight greenish tint and a linear contraction of 18.75 per cent. Mixture B was also translucent, but had a rather yellowish cast and a contraction of only 10.42 per cent.

These foregoing somewhat contradictory tests seem to indicate that the way in which the amorphous silica is imparted to the mixture has a distinct influence on its behavior in burning. The increased fusibility in one artificial test is not well supported by the second trial, but the behavior of the original clay which contains the free H_2SiO_3 can still be explained only on this hypothesis.

The lowest melting Seger Cone without Fe_2O_3 is cone No. 4, having the formula:

$$\left.\begin{matrix}0.3\ K_2O\\ 0.7\ CaO\end{matrix}\right\}0.5\ Al_2O_3,\ 4\ SiO_2$$

and in Seger's Gesammelte Schriften, page 191, there are given the following proportions for cone No. 4 mixture.

Rörstrand Spar	83.55 parts
Marble	35.00 "
Norway Quartz (Flint)	54.00 "
Zettlitz Kaolin	25.90 "

Seger treats these materials according to his analytical results as pure, and makes no correction for impurities.

A cone made according to these figures, corrected by the analytical results from the natural clay containing free hydrated silica acid melted exactly with cone No. 3.

Following the same principle, I made two mixtures of cone No. 4, in which I used in one the precipitated air dryed silicic acid, and in the other, ordinary dry ground flint.

MIXTURE A:		MIXTURE B:	
Spar	83.55	Spar	83.55
Whiting	35.00	Whiting	35.00
Amorph. SiO_2 cont. 43 per cent. H_2O	94.74	Dry ground flint (0 51 per cent. moisture)	54.27
Zettlitz kaolin	25.90	Zettlitz kaolin	25.90

Both were tested repeatedly, but I have to state that I could not discover any difference in the final melting point.

The shrinkage of Mixture A was again higher than that of Mixture B. Yet we must recollect that the naturally occurring clay containing free silicic acid depressed the melting point one whole cone number or about 20° C., when used in place of Zettlitz kaolin.

So far as I know, all ceramists who have tried to give a rule about fusibility have treated of silica only in its crystalline form. So does Bishop in his refractory quotient, and even Seger has based his theory only on the ratio of the fluxing constituents to the alumina and to silica. Of course, there is no clay containing free hydrated silicia acid among the German clays generally known up-to-date, but there is no doubt that not only the ratio of fluxing constituents to alumina and to silica, but also the state and condition of silica, whether crystalline or amorphous, has a far-reaching influence on the fusibility. I believe that we have in our country in those clays with free hydrated silicic acid, the means to manufacture porcelain at temperatures far below the heat applied in Europe, and I would not be surprised if the reason why Seger only partly succeeded in manufacturing porcelain at the same low temperature as the Japanese, would be found in the fact that Japanese use clay with free amorphous silica, while Seger had always to deal with silica in its crystalline form. Of course this is only hypothesis, as far as the Japanese are concerned.

For our American manufacturer, the lowering of the temperature in burning porcelain will prove to be of undoubted value in the future, as such a reduction of heat does not only mean the saving of fuel, but reduces at the same time the strain on saggars, their loss and last but not least it reduces the wear and tear of our kilns.

It is true that clay substance, freed of every bit of moisture and chemically combined water, is not plastic and that, besides shape and size of the smallest individual clay particles, the chemical composition is of the greatest importance for plasticity, so that we may say, that clays differ in a marked degree in plasticity according to the amount of interstitial water, of which there is an unusually large amount contained in clays with free hydrated silicic acid;

yet this very fact, which assists in reaching a very desirable degree of plasticity, will also prove a source of danger to the manufacturer.

It will and must increase shrinkage. A mass of clay may be looked upon as a labyrinth of pores, of capillary tubes, and while shrinkage is not only a phenomenon denoting a removing of water, but also indicates a consolidation, bringing the clay molecules nearer to each other, that is within the realm of more active attraction and action, still the potter has to be afraid of any excessive contraction, which may alter his shapes or cause his ware to warp in drying.

Crooked ware however is not the only danger. We know that water, especially when containing traces of alkalies, will dissolve hydrated silicic acid. In drying, the capillary tubes will carry this solution to the surface where the water will evaporate, while the remaining silicic acid will form a solid film, which will finally prevent an even drying, just as it will prevent the natural dry clay from "chalking" in water and sometimes will make it appear gritty and unplastic until the particles are torn apart by being forced through a sieve. The surface of a clay body with hydrated silicic acid will be dry already when the interior is still moist and the surface-tension caused thereby may become so great, that the ware will crack to pieces. This is especially dangerous in larger pieces of ware. At the first glance in making thin goods, the exceedingly quick hardening of the surface, the quick "letting loose" from the moulds, seems to be only an advantage, in the fact that the plaster paris moulds can be emptied quicker, and can be used more frequently.

The foregoing studies on these natural clays, and with synthetic mixtures designed to imitate them, have led us up to the following conclusions:

That the presence of any important quantity of free hydrated silicic acid in a clay tends:

First, to produce an increase of translucency over that obtained where the silica used is all quartzitic, at equal temperatures.

Second, to bring about an improvement in color.

Third, to increase their shrinkage both in air and in fire.

Fourth, to produce a lowering of the temperature at which vitrification occurs. This point was supported by tests of the natural clay, but only partly supported by synthetic mixtures.

Fifth, a tendency to warp in drying.

Sixth, a tendency to form a hard coating on the surface, due to the deposition of H_2SiO_3 from water used in making wares plastic.

If these few disadvantages can be regulated or overcome, it seems very likely that the American potter will find opening before him a perfectly new field, and that the clays containing free silicic acid will be very likely to become a very important factor in the development of true American hard spar-porcelain.

DISCUSSION.

Mr. Ernest Mayer: Dr. Zimmer has given us a very valuable paper, and I don't suppose there is a person in the room who can follow such an array of figures and be ready to discuss the paper at once.

The suggestion has been made that we discuss Mr. Burt's and Dr. Zimmer's papers together. I shall not attempt to discuss Dr. Zimmer's, for I will admit I do not thoroughly comprehend it yet. I want to see it in print before attempting to do so, as it will require some study. Mr. Burt's paper is right in line with the idea that I have been advancing ever since I have belonged to the American Ceramic Society and long before the Society was ever thought of.

When you speak of the *fine grinding of materials* I consider you are in the very heart of the greatest economic problem of the white ware business, because it has one of the greatest, if not the greatest, bearing on the question of reliable ware.

We all admit, I presume, that we mix our bodies and glazes in some kind of a systematic manner. In a body, for instance, a certain per cent. of this Feldspar, Ball and China clays are mixed together with a definite view in our minds as to what part each ingredient shall play.

Suppose a man making a body will leave it in his tanks over night and in the morning starts pumping out of the tanks right into the presses. It is impossible to tell what he is getting there; certainly it is not the body originally made, because the larger particles have settled down to the bottom. The English manufacturer takes his flint and stone in the liquid form. What would become of ours if we did that? We could not start our mills, if once they stopped. We could hardly dig them out. In England the flint and cornish stone is so finely ground, they can carry it for miles, and run it out into barrels, through small spigots. We could not do anything of that kind. Take a handful of our flint, mix it with water and while you are talking about it, it has settled down to the bottom in a hard mass. Take the English flint and mix it in water and it will stay there in suspension for a considerable length of time.

The Chair: Do you think that it is equally the case with French flint and rock flint?

Mr. Mayer: I don't think it makes any difference what kind of flint is used. Of course, all the flint they have in English potteries is from the Yorkshire coast — they don't know anything about rock flint. But if you will grind ours fine, it will act the same way theirs does. It simply means this: as soon as we grind our flint down properly, we will get the same results they do. But you can't get a flint maker in the United States to grind that way.

Mr. Edward C. Stover: How do they grind their flint in England?

Mr. Mayer: It is always wet ground.

Mr. Stover: The experiments I referred to in reference to grinding spar were made on a dry-grinding ball mill. Thomas Maddock & Sons explained the difficulty in getting a fine grind as a result of the use of a ball mill.

Mr. Burt: They can certainly grind as fine as they want to in a ball mill, if they grind wet.

Professor Edward Orton, Jr.: Mr. Mayer has made the statement, that he thinks the fine grinding of English flint makes it float in the water better?

Mr. Mayer: I do not know of any other explanation to offer. In England, if they go to a certain point in grinding it is all right, but if they stop too soon, it settles so hard they can't dig it out. In our materials, if we will grind and grind till very fine, we will have no trouble at all about this question of settling. But if it is coarse, it will settle, and we can't stop it.

Professor Orton: I wanted to state in regard to that point, the fact that Mr. Mayer has just brought out in his last remark, viz.: that between certain limits, that settling action takes place. I never made any experiments in regard to the fine grinding of flint, but I did, at one time, grind some window-glass down very fine. On one occasion I allowed it to set quiet awhile, and I had to take a chisel to get it up off from the bottom of the vessel containing it. It had woven itself into almost a cement on the bottom. I also found that the top of this cake consisted of the finest material which had remained longest in suspension, and it was not as hard to loosen up as the coarser material which underlay it. There was a clear line of demarkation between the two sediments. The line could be pretty readily seen between those particles which had settled readily, and those which settled more slowly. We could peel this top layer of fine material off in a sort of leathery film of a semi-plastic nature; that in the bottom, we had to dig out. It is also true, that if materials are coarse ground, we get from them no tenacity. It is only between certain limits of size of grain that this peculiar tendency to settle in such a hard cake takes place. Just what those limits are, I do not know, but I think the problem is one which we could easily solve by experiment.

Mr. Stover: In the preparatory work for your paper last year, the material was fine ground?

Professor Orton: No; not very. All of the sizes of material I was working with, at that time, would settle if left quiet for an hour or so. Compared to the material Mr. Mayer has described, my sands of last year were all coarse.

John W. Hasburg: I have not investigated the different varieties of flint or silica referred to by Mr. Burt by tests or comparisons thoroughly enough to make specific statements, but will say there is a great difference according to the source from which the silica is derived, whether it is mineral, organic, or chemical formation; also, if it is of a hydrous or anhydrous nature.

By comparing an opal or amorphous silica with rock crystal the physical difference is that the amorphous silica is a porous spongy material, while the rock crystal is a solid compact mass not at all porous.

Rock crystal, however fine it may be ground, will always be sharp solid crystals, while the amorphous variety will always be porous.

Caking or setting in the mill may be caused by hydrated silica. Some materials have the property of "setting" when mixed with a certain amount of water, but will not do so if an excess of water is used. If such a material is ground wet after having been mixed with water previously it will not set again.

Now if setting should be caused by hydrated silica it could easily be remedied by calcining. This will decompose the H_2SiO_3 to SiO_2 which will no longer be affected by water. Calcining does not destroy the porosity of the amorphous silica.

The "bond" of amorphous silicas is stronger than that of the rock crystal, also the amorphous silica is apt to fuse at a lower heat than rock crystal on account of the spongy structure which permits the other materials to work into the pores and effect combination more easily.

May this not be a probable explanation why in Europe they can fire at a lower heat than here?

Prof. Edward Orton, Jr.: There have been one or two points in my experience that would partly bear out Dr. Zimmer's work; also, some points in his work which throw some light on what I have not heretofore understood. Regarding this Indiana mineral, (I think Dr. Zimmer mentioned it) which is supposed to have the usual proportions of Al_2O_3 and SiO_2 and four molecules of H_2O, I would say that it has the peculiar conchoidal fracture which the flint fire clays all show. Why should both clays have this peculiarity in common?

And, two or three years ago, I received from Professor Dumble, formerly State Geologist of Texas, some material concerning which he wanted a report. We analyzed it, and found, as nearly as I can recollect, about 65 per cent. of silica, but it seemed excessively fine grained; when we burnt it in the kiln, we found that the raw material as delivered became glass-hard at cone 02. I have actually taken a piece of it and cut window glass nicely, after coming from a terra cotta kiln, fired to cone 02. Nothing had occurred in my previous experience to explain a case of that sort. It was not strongly discolored by iron, only to a pale yellow. There was no evidence whatever of lime; I tested for it by hydro-chloric acid, and got nothing. We then thought of hydrated silicic acid as the cause, and while my laboratory assistant was working on this theory, I wrote to Professor Dumble and asked the probable origin of the clay. He stated that the deposit was a peculiar one, extending in depth five or six feet over a considerable area of country; he viewed it as the decomposed remains of a volcanic ash deposit. He said he believed the country there had been pretty well covered over by a layer of very fine ash from volcanic eruptions in the mountain country west of there. There had evidently been a period of volcanic activity in Western Texas, but not in the present geological epoch.

The ashes ejected from volcanoes are merely fine particles of lava, not ashes in the usual sense. A great many volcanoes blow out great quantities of this lava-dust. Professor Whymper mentions a volcanic eruption in the Andes, S. A., where a column of dust thirty thousand feet high was sent up before it reached a current of air strong enough to overcome its vertical motion. Then, a strong wind broke the column off at right angles and carried it off across the country, falling in a fine shower of dust. So we see that volcanic dust may, under certain conditions, travel long distances in this excessively fine state. Now this dust is nothing more nor less than the same kind of rock which is at other times ejected from volcanoes as lava. That is, it is a silicate rock. If blown out as dust, it is certain to be chilled quickly; if it flows in great lava streams, it will probably cool slowly and crystalize into a coarse-grained rock. If it chills quickly, it will be exceptionally susceptible of attack by the processes by which kaolin is formed. So we see that such dust, deposited in a thin layer over a large area of ground, and subsequently weathering there, would tend to form a kaolin bed.

But, in the process of kaolinization of feldspathic silicates, we have never yet successfully accounted for the great quantity of silica which is set free, and of which kaolins ordinarily show no trace. For instance, feldspar may be considered to be

$$K_2O,\ Al_2O_3,\ 6\,SiO_2$$

In kaolinizing, we have assumed that the K_2O is converted to K_2CO_3 and taken up by water or by plants. Also, we have assumed that the Al_2O_3 unites with molecules of the silica and water to form

$$Al_2O_3,\ 2\,SiO_2,\ H_2O$$

But, four of the six molecules of SiO_2 originally in the spar are still unaccounted for. They may be taken up by plants; we know that some species, notably the bamboo, do store up quite a quantity in their tissues. Or the silica may crystalize again into quartz. This does not seem likely, because if it did, we ought to find some of the quartz crystals on the kaolin. We often do find plenty of quartz there, but generally as sand grains, which have obviously not crystalized where they are found, but have been formed by the grinding and working down of other quartz crystals of earlier formation. Or, water may dissolve it. But we know that only water containing alkaline salts dissolves silicic acid freely.

So, I repeat, that our explanation of the formation of kaolin has always been very lame. Now, when we find a clay, derived apparently from volcanic ash of comparatively recent geological age, and find it still lying *in situ*, just where it fell from the sky, and in a comparatively arid, poorly watered and poorly drained country, and find furthermore, that it contains a large amount of free hydrated silicic acid, I do not think we have occasion to feel any great surprise. It looks merely as if the SiO_2 has not had time or opportunity to disappear in that manner, whatever it may be, which it usually takes. It seems to me more surprising that this class of clays are not found oftener, instead of so seldom. In fact, I think many such would be found in our ordinary clays, if we were in the habit of looking for them.

I have enjoyed exceedingly Dr. Zimmer's presentation of this case, and it seems he has shown most ingeniously the effect of hydrated silicic acid upon the clay with which he has found it associated. This suggestion is also valuable to me, because I believe along this general line, we will find an explanation of the so-called flint clays, which we generally speak of as non-plastic. They become plastic, if at all, only upon excessively fine and long-continued grinding. We find them in the carboniferous strata in Ohio and in some other states in strata of other geological age. They are often practically pure kaolinites in composition, but also practically devoid of plasticity; the very antithesis of plasticity, in fact. They are so sharp, that they can cut the skin. The miners use gloves, to avoid being cut from flying chips of the clay.

I have examined out-crops of flint clays, where they lie near the surface, and have been weathering for no one knows how many centuries, and even where they have been ground by passing wagon-wheels to a fine, gritty sand; but even under these conditions they still show no signs of plasticity. We also find similar clays, with the same peculiar flinty structure, but containing five or ten per cent. of silica over and above what is necessary to form kaolinite with the alumina present.

We find these flint-clays interstratified with other fire-clays of the plastic variety; some times alternating in the same bed. In Ohio, they are invariably the under-clays of a coal formation, but I am told that in other states they are found with no coal or evidence of coal-forming conditions having been present when the clays were laid down.

Why should one clay be hard as flint and practically non-plastic, while another, formed within a foot of it, and not differing sensibly in its gross chemical composition, is plastic? This problem has long been vexing students of the clay industry and has yet to be solved.

But Dr. Zimmer speaks of this clay of his having a peculiar, hard, smooth surface and not becoming plastic until very finely ground. Other clays of the sort which I have seen, have this peculiar conchoidal fracture and inability to mix with water also, which characterizes the flint clay. It seems to me possible that many of our flint clays may be found to differ from other clays principally in the way their silica is present, rather than in its amount. There still remain some inconsistencies to explain, but, after all, there is a fruitful lead, it seems to me, in this idea of hydrated silicic acid and the development of the flint or non-plastic structure in clays. It is a fine field for one of our younger members to take up and explore in the light of the interesting developments which Dr. Zimmer has brought forward.

Another reason for thinking that hydrated silicic acid is formed under the conditions prevailing during the growth of the coal measures, in which these clays most plentifully occur, is that swamp conditions now act vigorously to decompose minerals which, in streams and running water, are not attacked. Swamps beget organic acids and mineral acids also by the decomposition of the organic matter which lies rotting on their slimy bottoms, and these acids are unknown factors in the decay and alteration of the rocks with which they come in contact. Sand-stone, exposed to swamp action for long periods, has been found superficially converted into amorphous quartzite by the agencies which it there met. If this is so, why should not clays also show the occasional conversion of their sands into hydrated silicic acid, and under the agency of this substance assume the hard, flinty and reluctantly plastic condition in which we find them?

I think Dr. Zimmer's paper is a strong addition to our literature, and congratulate him upon it.

Mr. Burt: Dr. Zimmer calls attention to the increased fusibility of his bodies from the fact of the silica being present in the amorphous state. It has occurred to me that perhaps that might have some connection with the composition of spar. It has always seemed peculiar that spar, with its high percentage of flux and low percentage or silica, should have nearly the same fusing point as Cornwall stone. Do you think the silica there might also be in the amorphous state?

Dr. Zimmer: I hardly think so. I did not investigate on that line. This body which I made almost on the line of natural Cornwall stone; about fifty per cent of spar, twenty-five per cent. of flint and twenty-five per cent. kaolin, or something like it. That mixture, melted, had the same appearance at cone number seven or eight as the natural Cornwall stone. It was almost identically the same.

Mr. William D. Gates: I want to say right here that I think this paper is one of the treasures of the Association, and if the Association passed away and had only produced one paper like this, it would have had sufficient excuse for existence. I shall await its appearance in print with the greatest interest.

The Chair: I would like to say before we close this discussion, how much I have enjoyed both papers. There are one or two points which I would like to bring before you. First, is the size of the grain, which I think Mr. Burt has demonstrated has a great influence upon the behavior of both body and glaze. I do not know that it is a point which any of us doubt very much; but still it is a very great satisfaction to have the thing properly demonstrated. The question of the behavior of the fine grained material, as Mr. Mayer has raised it, is also of the highest importance to all of us. Some regard the settling or not settling as evidence of fine grinding, and I regard it as such. The two papers hang together in this direction. The hydrated silicic acid is of course capable of almost perfect distribution in a body and after dehydration by the fire, plays the part of a finely ground material—in the potter's words, "it goes farther." I have examined some of these clays, and have been much surprised at their behavior, which I could not understand. It has now been made clear to me. The clays in North Carolina are granular, something like well-decomposed Cornwall stone, but become punky on grinding, and not plastic. These hard gritty clays, as has been shown, are similar to kaolinite in composition, but not in behavior. One becomes plastic and one does not. One point to which I want to allude is that made by Mr. Collins, whom Dr. Ries quoted last year, as to the production of kaolin by the action of hydrofluoric vapors. I think it was pointed out that while this was true of Cornwall clays, it was not true in America. This may show why we do not find hydrated silicic acid in Cornwall clays, and why we do find it in clays in this country—the fluoric action may account for this.

A METHOD FOR THE RATIONAL ANALYSIS OF CLAYS FOR CEMENT MANUFACTURE.*

BY

ALBERT V. BLEININGER, B. Sc.

The valuation of cement making materials is most generally based upon the results of chemical gross analysis. In this respect there is considered the fact that the alumina contents of the finished product must lie between the limits of 4.2 and 9.5 per cent., that of silica between 19.8 and 26.5 per cent., that of lime between 57 and 65 per cent., and that the magnesia contents should not exceed 3 per cent. Outside of these chemical limits there is a more or less well defined appreciation of the physical character of the materials, in regard to hardness, toughness, coarseness of grain, etc., but in the specific determination of the fitness of raw materials, some of the most important chemical and physical considerations are most frequently neglected. Since for the cement making process, the form of the silica and alumina which are to unite with the lime to form silicates and aluminates is of great importance in respect to the ease of perfecting the chemical union, the materials which furnish these constituents, the clays, should be put through tests determining their mineralogical and physical character. Thus a clay containing a large percentage of coarse silica in the shape of quartz grains, offers greater difficulties in the way of producing an intimate mixture, than one having the silica in the form of a silicate or a very fine dust.

Not only are the difficulties of manufacture to be considered, but also the fact that the quality of cement made from coarse clays is always apt to be inferior, owing to imperfect chemical combination, and in order to overcome this

---

*Printed by permission of Professor Edward Orton, Jr., State Geologist of Ohio, in advance of its appearance among the publications of the Ohio Geological Survey.

the capacity of expensive machinery must be reduced, involving extra wear and tear and an attendant increase in cost.

It is the object of this paper to summarize the methods hitherto employed for bringing out the physical and chemical characteristics of clays, and to offer a method intended to accomplish this in a practically feasible way, to the practical execution of which the Ohio Geological Survey has given its attention during the summer of 1900.

The problem of determining the physical and mineralogical character of clays has been attacked both from the strictly physical and the chemical side.

In briefly reviewing the physical methods we find two processes in the field, representing two methods of sizing particles, those of elutriation and sedimentation. The first, represented chiefly by the Schoene apparatus, is based on the fact that the carrying power of a current of water is a function of its velocity, and that hence a stream of water can carry larger particles, the greater its velocity. Thus, by varying the velocity from the lowest to the highest one adopted, a series of sizes of particles is obtained, and the clay being arbitrarily separated according to the sizes of its constituent grains. The Schoene apparatus, though able to give good results, is tedious in operation and not well suited for commercial work.

The second kind of mechanical analysis based on the principle of sedimentation is perhaps best represented by the Whitney process. This depends upon the fact that larger and therefore heavier particles, assuming a uniform specific gravity, when suspended in water, sink more rapidly than smaller ones. The grains, making up the clays, are in this way separated into any convenient number of sizes, usually about six, corresponding to different periods of suspension, checked by microscopic measurement. This method permits of accurate work when properly carried out, but it also suffers under the disadvantage of slowness of operation, and the importance of the personal factor in manipulation.

In regard to the value of these mechanical analyses there may be said, that though they furnish valuable data supplementary to the chemical analysis, yet alone they are lacking in a very important respect, since they simply separate the grains according to size, but do not give any information whatever in regard to their mineralogical character. Thus it is essential to know whether a certain size of grain is composed of particles of quartz or feldspar, which, owing to their chemical character, behave quite differently in contact with lime at a high temperature.

Of the chemical methods there are to be mentioned the methods of rational analyses as devised by Prof. Seger and its modifications by Prof. Fresenius (Quant. Analysis, Vol. II, page 352,) and Dr. Heintzel, and a method suggested by Prof. Lunge and Dr. Schochor. These will now be taken up in the order named.

In brief, Prof. Seger makes use of the fact that boiling concentrated sulphuric acid dissolves that portion of a clay corresponding more or less to the so-called clay substance, but does not to any extent attack feldspar and quartz, the other two essential constituents of clay. Hence by treating with smoking sulphuric acid and following this by a treatment with hot carbonate of soda for the removal of the gelatinous silica, it is possible to remove the clay substance (approximately Al_2O_3, 2 SiO_2, 2 H_2O) from the mineral detritus, feldspar, quartz and micaceous minerals. From the analysis of this residue, the per cent. of feldspathic minerals and quartz can be calculated, using the ratio of silica to alumina in feldspar as a basis.

This method, though giving extremely valuable data in regard to the mineralogical character of a clay, yet neglects to give expression to the coarseness or fineness of the grains composing the matter insoluble in acid, and hence fails to bring out the degree to which the clay unites with the lime to form the silicates and aluminates of lime. In order to overcome this to a certain extent, a mechanical analysis would have to be made of the insoluble residue, which, however, would still fail to tell just which of the various sizes are feldspar and which quartz. Lime can decompose and unite with the constituents of feldspar, the latter being a silicate, with far greater ease than with quartz, both being assumed as belonging to the same size of grain. Hence we see that the ordinary rational analysis is deficient and does not produce exactly the kind of data desired.

The Fresenius and Heintzel methods are especially intended to show the fitness of calcareous clays, that is, marls, for cement purposes and assume that the solubility of the raw material in acid is a criterion of its value. This opinion, though accepted by a number of chemists, is erroneous, as has been amply shown by Prof. Lunge and Dr. Schochor.

Lunge and Schochor proceed on an entirely different basis and make use of the fact, more akin to the purpose in view, that silicates are unlocked by being heated in content with CaO, as is clearly shown by the J. Lawrence Smith method of alkali determination. These investigators, though they did not work with clays proper, but with marls, succeeded in showing that the CaO attacks most readily the combined silica and the silica present in a fine state of division, thus giving a direct expression of the cement-making value of the materials. By heating sufficiently, treating with hydrochloric acid and sodium carbonate the unlocked bases and the soluble silicic acid are removed, and the residue remaining is simply the matter which was not rendered soluble by the action of the CaO during the heating process.

The work by Messrs. Lunge and Schochor in regard to this investigation was very extensive, and, before adopting their final method they made very thorough preliminary tests. The results obtained by them may be summed up as follows:

1. The solubility of raw marls in acid is not a sufficient criterion for determining their hydraulic value because acids are not proper regents for determining unlocked silica, and since silicic acid itself is soluble in acid only in proportion to the amount of chemical water contained by it. The acid treatment does not discriminate between coarse and fine

silica, between amorphous silica and crystaline silica, nor between feldspar and silica.

2. However amorphous silica and quartz may differ otherwise, they show but little variation in behavior in burning with lime. The fineness of grinding is far more important. Although with the same degree of fineness and the same mode of mixing with equal amounts of CaO and heating under similar conditions, amorphous silica gives rise to a higher percentage of soluble silica than the quartz or a mixture of the two kinds of acid, yet when ground sufficiently fine, even pure quartz is acted upon almost completely and much more so than amorphous silica not so fine, but still extremely fine from the ordinary standpoint. Since the activity of silicic acid depends upon its contents of chemical water, after ignition, it does not differ from so much quartz.

3. Chemically combined silica is almost completely unlocked. No matter how fine free silica is, it is always attacked less than silica in combination and hence it is shown that the best source of silica for cement purposes is a silicate, as far as transportation of the silica to the line is concerned.

Lunge and Schochor have thus indicated a way by means of which it is possible to determine the per cent. of chemically active material in a clay, to separate the combined and very fine silica from the coarser quartz, permitting the statement that the higher the per cent. of silicate and very fine quartz is in a clay the higher is the value of the latter for hydraulic purposes. In other words, the higher the per cent. of dissolved material after heating with CaO and treating with HCl and Na_2CO_3 the higher is the value of the clay for hydraulic cements.

The method of working adopted by the two investigators named was as follows:

Samples of marl were taken, in the shape of lumps of walnut size which were heated in a gas furnace to a temperature of about 1100° C. without pulverizing or mixing, for two hours. These burned samples were pulverized, treated with HCl and Na_2CO_3 the residue being weighed after thorough washing. The value of the respective marls was expressed inversely by the per cent. of residue.

In order to prove that these tests really stand for expressions of the hydraulic value of the marls, Lunge and Schochor proceeded to grind the materials and to make cements of them by burning to vitrification and grinding the resulting clinkers. The burning was done in a small test kiln. The hydraulic cements thus produced were subjected to the usual cement tests, which proved strikingly that for the marls examined, the hydraulic value was a function of the solubility of the clayey constituents when treated as mentioned above.

Upon studying the results obtained by Lunge and Schochor, Prof. Edward Orton, jr., suggested the use of this method for the testing of clays in regard to their suitability for cement manufacture. The investigators just named restricted their method to marls, and although it is extremely valuable for the determination of the hydraulic value of any one marl, yet its results are not generally applicable and must always be restricted to this class of material. The factors in this method are evidently the variables of the clay substance, coarse and fine quartz, coarse and fine feldspathic matter, and in addition to this the lime contents. This evidently may in some cases give rise to conflicting results, as for instance, when there are two marls to be examined of which one has a coarser grain of sand but a higher contents of lime than the second marl, which is fine grained and has a higher percentage of combined silica but a lower per cent. of lime. The method now suggested for clays adopts a constant amount of lime, thus eliminating one variable.

The identity of structure which Lunge and Schohor are so careful to preserve by burning the materials in lumps is not interfered with by reducing them, *without grinding*, to the size of a 40 mesh sieve. In fact by burning the materials in lumps the outside factor of irregularity due to natural formation is so great as to introduce an additional source of error. For a product as homogeneous as cement the consideration of the bed structure is entirely unnecessary.

Thus the process of the Ohio Geological Survey is intended to determine the hydraulic factor of a clay by reducing the latter without grinding, with the assistance of water, so that it will pass through a 40 mesh sieve, care being taken not to reject the coarser material left on the sieve nor to break up any particles by grinding. When in the state of a slurry, lime is added in the shape of precipitated calcium carbonate and thoroughly stirred with the clay. This mass is now dried down and rubbed through a 40 mesh sieve, mixed thoroughly on a sheet of glazed paper by means of spatula and burned in a furnace. A weighed amount of the ignited mixture is treated with hot hydrochloric acid for five hours, filtered and washed, and the residue is treated with a 10 per cent. solution of sodium carbonate under heating, until no more flocculent silica is observed on testing with ammonium chloride.

The Lunge and Shochor method and that of the Ohio Geological Survey differ in the following respects:

1. The above named investigators restrict their method to marls, while the Survey extends it to the testing of all clays.

2. The former work with the natural materials, having varying percentages of lime, while the latter maintains a constant amount of lime by adding the lime lacking, in the form of pure calcium carbonate and also having the lime present in excess.

3. Lunge and Schochor ignite the materials in the shape of lumps taken from the deposit; the Survey reduces them so that the soft portion will pass through a 40 mesh sieve with the aid of water and accomplishing the mixture of clay and lime when the former is in the condition of a slurry.

Having thus outlined the method of working, there now remains to prove that it really gives expression to a co-efficient which is consistent with actual results in the manufacture of cement. For this purpose it was decided to bring the method as nearly possible to uniform conditions of operation and to agree upon a method of mixing, burning and analysis, and finally to make cements out of the materials thus analyzed, which are to be tested according to the usual specifications for Portland cements.

The description of the work done naturally falls under the following headings :

1. Sampling and preparation.
2. Mixing.
3. Burning.

SAMPLING AND PREPARATION.

The clays arrive at the laboratory of the Ohio State University Ceramic Department in sacks, in the shape in which they were sent by the field collector. These clays, according to their nature, must be treated in different ways. Clays of the type of soft, alluvial clays, after being put through an 8-mesh sieve and repeatedly quartered according to the quantity on hand, are dried at 100 degrees C. and about 500 g, is reserved for the analysis, being run, as far as possible, through a 40-mesh sieve and thoroughly mixed ; the coarser portion is, of course, added to the finer. If the material is a hard shale, it is necessary to break it up to the size corresponding to an 8-mesh sieve. A quantity of the reduced shale is now heated at 100 degrees C. till perfectly dry and then allowed to soak in water for 24 hours. If this is not sufficient, boiling is resorted to, which is kept up for 6 to 8 hours, if necessary. The mass is now evaporated to dryness and rubbed through a 40-mesh sieve. For very hard material, rubbing with a rubber pestle may be permitted.

MIXING.

The sample is now ready to be mixed with the lime. This is done by weighing out, say 25 grains of the clay to be examined, whose gross analysis must be known, stirring it up with water to a thin slip and adding reprecipitated calcium carbonate. This mixture is to be stirred thoroughly, evaporated to dryness, rubbed through a 20-mesh sieve and thoroughly mixed with a spatula.

At this stage of the work it was necessary to determine the proper amount of lime to be added to a clay so as to attain the object sought, that is, the decomposition of the clay substance proper and the formation of a lime silicate, without running the danger of fusing the resulting mixture at the temperature employed in heating. In order to accomplish this a series of mixtures was made, employing a very fine-grained, soft clay, with little bonding power which is used in the manufacture of Portland cement, at Harper, Ohio. The clay has the following analysis:

SiO_2	50.70
$Al_2O_3+Fe_2O_3$	17.00
$CaCO_3$	16.80
$MgCO_3$	9.80

The clay by itself was made the start of the series and the lime was increased as shown by the following table. The mixtures were burned at a temperature of 1100 degrees C, this heat being maintained for 75 minutes.

No.	Per Cent. of Lime Carbonate	Per Cent. Residue	Remarks
1	16.8	....	Clay alone.
2	25.	....	
3	30.	....	
4	35.	....	More or less completely fused.
5	40.	....	
6	45.	....	
7	50.	....	
8	55.	....	Vitrified.
9	60.	3.55	Quite hard.
10	65.	....	Hard.
11	75.	1.12	Soft, but somewhat too hard for easy reduction.
12	80.	2.16	Soft.
13	85.	3.00	Soft and friable.

The mixtures of clay and calcium carbonate were made up into briquets weighing about 75 grams, with the aid of water, placed upon a biscuit plate and burned in the small furnace used for this purpose. No. 11 of the table must be considered doubtful, an error having certainly been made in the analytical work. The results of the table show that it is essential to have at least 75 per cent. of calcium carbonate present in the mixture, not so much because less lime might not unlock the silicate or not attack the free silica, but owing to the fact that the resulting mass after burning becomes hard and stone-like and hence inconvenient for reduction. For this reason it was decided to use a standard mixture of 80 per cent. $CaCO_3$ and 20 per cent. of the clay to be tested, which results in a friable mass easily reduced. This amount of lime also allows for a sufficient excess, so that uniformity is obtained as far as the reagent is concerned.

In the determination of the insoluble residue of these mixtures sufficient material was taken to correspond to two grams of the clay. In this and other series the total lime was determined volumetrically in a number of analyses. This was done in order to express the per cent. of residue in terms of the actual weight of clay taken, thus making due allowance for the lime introduced as reagent, since the mixing of clay and lime can never be so intimate as to make it permissible to weigh out any amount of the mixture and obtain the actual amount of clay taken by calculation, depending upon the per cent. of calcium carbonate originally added under the supposition that the mixture is perfect. Later on this method was modified in such a way that a 2 grams of the dried clay were weighed out, transferred to a beaker and made up into slip with water. To this was added the amount of lime corresponding to the required percentage. The mixture was carefully transferred to a porcelain crucible, evaporated to dryness, mixed on a sheet of glazed paper with the spatula and returned to the crucible in which the sample was burned, being covered with a lid. In this way, of course, the determination of lime becomes unnecessary.

The analysis proper was carried out as already indicated. Two grams of sample were taken and treated with 40 c. c. hydrochloric acid (1:3), allowing to boil for five minutes till in solution. The solution is now filtered and washed till nearly free from chlorine, the residue being washed back into the beaker and treated with sodium carbonate solution (8 grams per 100 c. c.), 100 c. c. being used each time. The treatment is continued till no more precipitation occurs with NH_4Cl. The residue is then treated with hydrochloric acid (1:4), washed free from chlorine, ignited and weighed.

The main precaution to be observed in this work is to work as hot as possible, but yet allowing the silicic acid to settle. The use of long stemmed funnels is to be preferred to the suction flasks.

The analytical work was done by Mr. Ralph W. Nauss, assistant on the Ohio Geological Survey.

BURNING.

For burning the mixture, there was available a dental furnace manufactured by the American Gas Furnace Co. which was intended to be fired with gasoline vapor, but which was modified so as to burn ordinary kerosine. For this purpose a ¼ inch pipe was introduced into the lower cylindrical part of the furnace. The pipe was run to a can fastened to the wall and was fed with oil from the reservoir by means of a stop cock and funnel. The oil dripped into the open furnace making a sight feed arrangement. The blast was furnished by a positive rotary blower through a ¾ inch pipe, operated during the summer by a gasoline engine.

The mode of operating the furnace was as follows: At the bottom of the cylinder beneath the furnace proper there was placed a small shallow iron pan into which the oil dripped from the feed pipe and which served as an evaporator for the oil. When ready for operation a small bunch of waste saturated with oil was placed upon the pan, ignited and covered with a few pieces of wood and charcoal. Upon this small charge of fuel there were now placed pieces of fire brick, the size of a large walnut, which filled up the cylinder completely and served the purpose of offering resistance to the passage of gases and air, thus mixing them for proper combustion. The air was blown in through three small tuyeres distributed around the cylinder. As soon as the charcoal is fully ignited, the kerosene is allowed to flow slowly and a weak blast is maintained. Gradually the flow of oil and the blast are increased, until the maximum supply is reached.

Before the supply of oil has commenced, a fire-clay tile is now placed across the furnace occupying the space formerly taken up by the muffle. Upon this tile are placed the crucibles or biscuit saucers carrying the samples to be ignited. At the same time a porcelain tube containing the platinum wire and thermo-couple of a Chatelier pyrometer is placed in position, just touching the mixtures to be heated.

The rise in temperature is readily followed by means of the pyrometer whose millivolt meter is set up in a closet at a distance of about 7 feet from the furnace.

The question now arising is in regard to the temperature to be employed in the burning. It is at once evident that the work to be accomplished by the heat is two-fold.

First, the carbonic acid must be expelled from the calcium carbonate.

Second, the silicate must be decomposed and the finely divided silica converted into lime silicate, soluble in acid.

The first function obviously fixes the lower limit of the temperature to be employed. From the work done by Prof. Hertzfeld, we know that at a temperature of 1050 degrees C the carbonic acid is completely expelled. Lunge and Schochor employed a temperature between 1060 and 1120 degrees Centigrade, their statements being in this respect not as definite as could be desired. However, from the extensive knowledge in existence in regard to the unlocking of silicates by the J. Lawrence Smith process, we know that it is not necessary to carry the temperature very high and this is also not desirable owing to other considerations. The heating must not be carried so high that silicates and aluminates analogous to those of hydraulic cements are formed, reactions which are entirely uncalled for in this process. We thus have the limits of temperature fixed, the lower one being that of the complete expulsion of carbonic acid and the higher one being that of the formation of hydraulic silicates and aluminates.

It was not thought necessary in view of the knowledge available, to undertake an extended series of experiments as to the temperature required. But it was decided to heat a mixture of clay and carbonate of lime at different temperatures and to observe at what temperature the residue becomes constant. For this purpose a marl, from Harper, Ohio, was selected, whose composition is as follows:

SiO_2	8.75
$Al_2O_3+Fe_2O_3$	2.78
$CaCO_3$	79.22
$MgCO_3$	2.67

A sample of this material was heated to 1000, 1050, 1100 and 1150 degrees Centigrade with the results as shown by the following figures, the time chosen being 90 minutes, that is, the respective temperature was maintained for this time. The amount of marl taken for analysis was 2 grams.

Temperature,	1000°	1050°	1100°	1150°
Residue	2.06	1.00	0.4	...

The results were found to check sufficiently close in other burns. The samples were made up into briquets.

It is thus seen that practically all the marl is decomposed at 1100 degrees. Although this series might have been extended to mixtures of clay and carbonate of lime, yet the fact that this marl, which is acknowledged to be a most excellent cement material, was practically decomposed, tegether with the data obtained from the work of Lunge and Schochor, Hertzfeld, Smith, seems sufficient to warrant the acceptance of this temperature as the standard.

The question of the time of heating was one that next needed attention. In order to determine the factor of time, the same marl was used and heated for 30, 60, 75, 90 and 120 minutes at the temperature of 1100 degrees C.

Time	30	60	75	90	120 minutes.
Residue	8.6	1.28	0.4	1.0	0.55

In spite of the irregularity in the 90-minute burn, which shows an increase of 0.6 per cent. of the residue, it was decided to adopt 75 minutes as the standard length of the rational burn. The irregularity may be explained by the variations necessarily co-incident with the nature of all rational analyses of this kind, which do not work upon finely pulverized, homogeneous samples.

By 75 minutes is meant the time during which the sample is exposed to the temperature of 1100 degrees C., which, of course, means a much longer burn, since one hour elapses before the furnace can be raised to 1100 degrees.

There can be little doubt but that the time thus allowed is ample to accomplish the intended result. It is well known that for the complete decomposition of silicates in the alkali determination, a heating of 50 minutes over a Runsen burner is sufficient, the temperature of which certainly does not exceed 1100 degrees.

Having thus arrived at standard conditions of burning which, in the nature of the case, are bound to be more or less arbitrary, it was decided to apply this method to typical constituents of clay, prepared in several well-graded sizes. For, by knowing the action of lime upon each of these materials and for each of several sizes of grain, it is possible for us to obtain a more definite idea of the character of the more inert constituents of clay, not readily available for hydraulic purposes. Of course, we can say in general that the coarser the particles of quartz and feldspathic and micaceous minerals, the more resistant will they prove to the action of lime, yet we do not know just to what extent the chemical action of lime is affected by the size of grain.

For this reason two series of experiments were carried out in which quartz and feldspar grains were burned together with lime in a mixture containing 80 per cent. of the calcium carbonate, the heat being maintained for 75 minutes at a temperature of 1100 degrees C.

The quartz taken for this purpose was pure quartzite which was crushed and graded to the sizes by sieving. Six sizes were adopted, as follows:

1. Grains between 150 and 120 mesh sieve.
2. Grains between 120 and 100 mesh sieve.
3. Grains between 100 and 80 mesh sieve.
4. Grains between 80 and 60 mesh sieve.
5. Grains between 60 and 40 mesh sieve.
6. Grains between 40 and 20 mesh sieve.

In addition to these sizes, there was included in the series a ground flint, as sold to potters, which is quartzite ground to a fine powder in ball mills using flint pebbles.

After being mixed with $CaCO_3$ in the condition of a slip, the mixture was evaporated to dryness and made into briquettes which were burned.

On making the rational analysis the following results were obtained, as shown by the table:

SIZES	Ground Flint	150-120	120-100	100-80	80-60	60-40	40-20
Per cent. residue	28.83	63.8	78.53	86.52	86.27	96.22	96.83
Per cent. taken in solution	71.17	36.2	21.47	13.48	13.73	3.78	3.17

This table shows quite strikingly the effect of size of grain upon the action of lime, and brings out two kinds of facts which have a direct bearing upon the cement industry.

It confirms the general statement that the coarser the grain, the smaller the effect of the lime. Free silica contained in clay, unless very fine, finer than that passing through a 150-mesh sieve, is rendered available only after thorough grinding, which means a certain extra amount of wear and tear of machinery, and a decrease in efficiency corresponding with the coarseness of the quartz.

The second fact is to a certain extent opposite in character to the first. It tells us that it is commercially possible to reduce free quartz to a fineness in which it is available for combination with lime. In order to be more clear. it may be said that of two clays available, one high in alumina and low in quartz, and one with a proper alumina content, but rather high in free silica, the results in regard to the quality of cement will certainly be better when the silicious material is used, in spite of the fact that its grinding is more difficult.

The tendency, only too common, is to work softer clays, which often bring in more alumina, and result in a tendency to "hot" cements, which are toned down by calcium sulphate. Granted that the presence of quartz makes it necessary to grind longer, yet the clay should not altogether be chosen with regard to its easy reduction, but also in regard to the ultimate quality of the cement.

Similarly to the quartz series, the same process was carried through with the corresponding sizes of feldspar grains, which were obtained from several fine specimens of orthoclase crystals, that apparently had not been kaolinized preceptibly.

SIZES.	Ground Feldspar	150-120	120-100	100-80	80-60	60-40	40-20
Per cent. residue.....	3.75	15.45	31.00	64.29	79.63	95.72	....
Per cent. taken in solution	96 25	84.55	69.00	35.71	20.37	4.28	

These results show at once the great difference between quartz and feldspar in regard to being acted upon by lime. It becomes evident that feldspar is a desirable constituent of cement materials. Light-burning clays high in feldspar should therefore receive more attention with reference to the manufacture of light-colored cement.

In addition to these series, commercial potter's kaolin of a good grade was treated by the same process, resulting in a residue of 3.07 per cent., 96.93 per cent. being taken into solution. This verifies the work of the German investigators, who found that clay substance was easily unlocked by lime.

Finally a synthetic mixture, having the following composition, was prepared with the intention of closely imitating such a clay as is commonly found in the alluvial or glacial clays of the country, and as is likely to be tried as a source of silica and alumina in cement manufacture.

Commercial, washed Kaolin	50 per cent.
Feldspar, 150–120 mesh	10 per cent.
Feldspar, 120–100 mesh	10 per cent.
Quartz, 150–120 mesh	10 per cent.
Quartz, 100–80 mesh	10 per cent.
Quartz, 60–40 mesh	10 per cent.
	100 per cent.

This mixture would have a composition on analysis roughly as follows:

SiO_2	65.50
Al_2O_3	24.00
K_2O	3.00
H_2O	7.50
	100.00

Upon being heated with lime it showed, on analysis, a residue of 31.36 per cent., 68.64 per cent. having been taken into solution. The residue, calculated from the previous experiments, should be 30.72 per cent., which is a practical check on the previous work.

The foregoing method has yet to be subjected to the final test of practical application. Not until we have demonstrated that the clays which this method condemns, are also condemned after actual use as cement materials, and that clays which this method endorses as good, are actually found good in use, can we treat it as an established and valuable thing.

However, its claims to usefulness rest on broad and secure technical reasoning. No one can successfully dispute that clays, which give up their silica most readily to the action of lime, are, other things being equal, the best for making cements. Neither can any one affirm that any other method yet proposed or used, has been successful in showing any close relation between the amount of sandy minerals in a clay and its value to the cement-maker.

The clay must contain other sources of silica than that of kaolinite, if it is to be serviceable to the cement-maker, and, in addition, these sources must be such as will readily yield it up. The older methods have stopped after showing that the requisite silicate matter is present. This proposed method is designed to go one step farther, in showing not only that it is present quantitatively, but also whether it is qualitatively suitable as well. It shows whether the silicate matter which is present, is in a form in which it will be readily assimilated into the calcium silicates to be formed, or whether it is in such physically unfit condition that only a limited part of it will be useful.

In the latter case, using the data furnished by the older methods, the cement-maker is likely to produce a cement too poor in combined silica. Or, if he recognizes this danger, and uses an excess of clay to bring the proportion of available silica up to the proper point, he runs into the counter difficulty of bringing in too much alumina.

When the experimental work, which the Survey has begun, is completed, it is hoped that further data can be given, showing the value of this process in actual work.

DISCUSSION.

N. B.—The paper was read by the Secretary, in the absence of the writer.

Mr. E. C. Stover: Professor Orton has probably been familiar more or less with these experiments, and I would like to ask him a question. In making the burns, was the product colored by soot, or was it burned free from discoloration?

Professor Edward Orton, Jr.: The lime silicates which have been produced so far have none of them blackened until they reached the cement-forming stage. I don't think they get any black coloration because of carbon taken up from the flame. The burn is of a very intense nature, and almost bound to be reducing in character, and it will almost necessarily bring all iron in the cement into the ferrous condition. If we could get a muffle up to that temperature, it would be more easily possible to produce oxydizing conditions at the same time. But, you have got a mixture of sixty-five per cent. of calcium oxide to combine with only twenty-five per cent. of silica, and it takes a higher heat than can be attained in any ordinary muffle. I think the dark color of the clinker produced, is due to the reducing character of the burn and not to carbon.

Mr. Stover: Can you tell what heat has been used? What cone it is equal to?

Professor Orton: A German investigator recently tried a cone test in one of the modern rotary cement kilns. He made a cannon-ball arrangement out of fire-brick, making a hollow ball-shaped affair in two pieces fitting them together so they would clamp or key together; inside was a number of cones. They started this spherical fire-brick bomb in at one end of the rotary, and let it go through and come out at the other end. It took about an hour and a half to go through, and cone eighteen was melted, I believe.

A friend of mine put some cones into a small stove apparatus he was using for burning cement; he put in numbers five, ten, fifteen and twenty. Ten was melted to a perfectly clear glass; fifteen was *melted into an opaque white slag;* and cone twenty was bent and twisted, but on breaking it to examine the cross section, was found to be only nicely vitrified. This little test shows that a temperature between cones 15 and 20 was reached in burning cement in an experimental way.

Mr. Stover: That explains why I could not make cement. I made one or two cement tests in my biscuit-kilns, and as we fire at cone eight or ten in Trenton, we could not get sufficient heat to make a clinker. The material came out spongy and unvitrified.

THE CLASSIFICATION AND NOMENCLATURE OF THE SILICATES.

BY

EDWARD ORTON, JR., E. M.

One year ago, a very able paper was read before this society on the subject of "Monosilicate Glazes." Simple as this title is, it is somewhat strange that it should have occasioned or permitted any misconstruction or misunderstanding, especially among a body as well acquainted socially and professionally as is the case with members of this society. But, such is the unfortunate condition of chemical technology at this time, especially of the ceramic branch, that some question did arise, as to what this title meant. Some thought that the monosilicate referred to meant a silicate containing one base only; others thought it meant a silicate computed by the oxygen ratio; others construed it in the sense in which the author really used it.

The citation of this illustration must not be construed by any one as a reflection on the choice of this title, or the propriety of its application. In fact, no other could have been used, which would not, in this society at least, have been open still to the same misconstruction. It is cited merely as a fresh instance of the fact with which all of us are familiar, and which is a constant source of trouble and embarrassment to some of us at least, viz.: that the various branches of chemical technology are using different systems of silicate nomenclature.

This confusion arises from several causes. To begin with, the naming of the silicates is really a matter which falls to the chemist. To him belongs the first right to discuss this problem, and from his conclusions we should be slow in departing. It is through the chemist that we have received our views. They are the pioneers in bringing the degree of order which exists at present, from the chaos of preceding years, and naturally they are factors whose views in the settlement of such questions cannot be ignored.

But chemistry is a very rapidly growing science. It has only been taught in the colleges with any approach to fullness and thoroughness for some fifty years, and the number of trained chemical observers and thinkers has probably increased an hundred fold since 1870; at least, this is true for the United States. Moreover, chemistry is a science of infinite detail. The facts which make up the sum of modern chemical knowledge are simply legion, and it still seems as if our knowledge is capable of infinite extension in the same direction. The mass of recorded information has grown to be so great, that no man's life-time is sufficient to make him acquainted with it, and yet we seem as far from the end of experimental research and deduction as we were twenty-five years ago. In fact, at no time in the past, have such an army of trained, cultured observers been at work, as those that are now eagerly probing into the unknown portions of the chemical field.

As is natural in a rapidly-growing science, our idea of the philosophy of chemical combination, of quantivalence, and of structure, have changed many times, and doubtless will change still further. The chemist of fifty years ago read his formulæ quite differently from the way that we now employ; his system of atomic weights were different; his conceptions of chemical union were different. He knew nothing of the periodic law, of ionization, or of physical chemistry in general. And while the change has been for the better, and while the present conception of chemistry affords a much more comprehensive view than ever before, it is too new as yet to have been adopted in all of the branches of the chemical industries.

Herein lies the trouble. The advance of pure chemistry has been more rapid than that of the technical industries, which depend upon it. The bold outlines of chemistry, and the fundamental method of doing things, have not changed materially in these fifty years. Men knew how to make certain chemical products as well then as we do now; hence, this work has been relegated to a class of men whose tastes lie in doing well the things they understand, rather than in investigating unknown fields, or busying their minds with speculations on the philosophy of the subject. In other words, the investigator has out-stripped the technicist, and from this fact grows the present confusion.

THE MINERALOGICAL CLASSIFICATION.

Mineralogy is one of those branches of chemical study that have been largely created during these fifty years, and its evolution has followed and in a measure kept pace with that of pure chemistry.

But the mineralogist is not merely a chemist. He has other problems on hand, which sometimes seem to him more important than the chemical constitution of his minerals. The latter are often curiously complex, and chemical analyses have been most unsatisfactory as a means of solving them; while physical means, principally the microscope, polariscope, and sectioning lathe, have shown up methods of attack which have proven much more fruitful. Hence, the mineralogist, while in the main a good chemist, has generally been somewhat behind hand, and his conceptions of mineral structure have not always expressed the latest chemical views. For instance, Dr. Bruno Kerl, in his "Thonwaaren Industrie," cites formulæ for kaolinite from various preceding mineralogists, which give a half dozen different constructions to this simple mineral.

At one stage of their work the mineralogists used a very complex system of formula-writing, which cannot here even be reproduced without the manufacture of special type for the purpose. It is certainly true that no one, possessing merely an ordinary knowledge of chemistry, would understand at first what one of these old mineralogical formulæ meant.

The mineralogist has, for a long time, been puzzled to explain the apparent anomalies in mineral composition. For instance, he finds a number of minerals, all of the same crystaline system, all having some characteristic physical points in common and yet a most annoying divergence in composition. One, only, of the group may perhaps have a simple chemical structure in which one base combines with one acid. Others will show two bases, the sum of whose equivalents equal the first base in the simple type. Others again will show three, or even four bases, the sum of whose equivalents is merely enough to saturate the original quantity of acid atoms.

For instance, let me quote the composition of some of the pyroxene group.

TABLE I.

NAME	New Chemical Formula	Old Method of Writing Same Formula.
Pyroxene type.......	$RSiO_3$	RO, SiO_2
Malacolite...........	$CaMgSi_2O_6$	$CaO, MgO, 2\ SiO_2$
Malacolite, var.	$CaMg_2Si_3O_9$	$CaO, 2\ MgO, 3\ SiO_2$
Sahlite................	$CaMgFeSi_3O_9$	$CaO, MgO, FeO, 3\ SiO_2$
Sahlite, var.	$Ca_3Mg_3FeSi_7O_{21}$	$3\ CaO, 3\ MgO, FeO, 7\ SiO_2$
Sahlite, var.	$Ca_2Mg_2FeSi_5O_{15}$	$2\ CaO, 2\ MgO, FeO, 5\ SiO_2$
Sahlite, var.	$Ca_4Mg_3FeSi_8O_{24}$	$4\ CaO, 3\ MgO, FeO, 8\ SiO_2$
Hedenbergerite......	$CaFeSi_2O_6$	$CaO, FeO, 2\ SiO_2$

There are also Lime-Iron-Manganese-Zinc Pyroxenes or *Jeffersonite*, Alumina-Lime-Magnesia Pyroxenes or *Leucaugite*, Alumina-Lime-Magnesia-Iron Pyroxenes or *Augite*, and others whose composition is still a matter of doubt, because it seems impossible to obtain two samples of a composition uniform in all details. At the conclusion of this list, the author (Dana) naively remarks that these groups shade so insensibly, one into another, that it is difficult to classify them accurately.

The hopelessness of finding any rational explanation to this chaotic profusion of chemical compositions, in minerals which have almost everything else in common, was only increased by the attempts of chemists to have mineralogists adopt their newer nomenclature as shown in column 2 of the above table. They were asked to give up the detailed and analytic formulæ in column 3, because these no longer represented the newer conception of the methods of chemical combination. It is easily apparent that the change was not one which conduced to a clearer understanding of structure, as shown in these complex minerals. The mineralogists loyally attempted to do it, but the entire lack of the graphic character in the new nomenclature has stood as a permanent bar to progress. Dana devised what he calls the "quantivalent ratio" to get around this difficulty, in which he uses the total quantivalence of the basic *vs.* the acidic members of a mineral compound. But this device does not really meet the fundamental objection of the chemists at all; it is only a parallel way of doing exactly what the oxygen ratio had been before designed to do, and the old fundamental conception of molecular union, instead of atomic union, was still there in unchanged strength. In fact, *it had to be there*, to enable the mineralogists to work. Without it, they were lost, for the new method gave them at that time nothing in its place.

A great change has begun to take place in recent years in this matter, through the attempts of mineralogical chemists to apply to inorganic salts, especially the silicates, the same sort of graphic formulæ which have long since proven so valuable in organic work.

Dr. F. W. Clarke's luminous exposition on "The Constitution of Silicates" (U. S. Geol. Survey Bull., 125,) is a most valuable example of what is being done in this line. Here, the silicates are all considered as salts of the five known silicic acids, and by means of the graphic structural formulæ, it begins to be more clearly seen how a mineral may present so kalideoscopic a composition, while all the while retaining a type structure and definite mineral characteristics.

THE METALLURGICAL CLASSIFICATION.

Metallurgy is another of the branches of science which also deals extensively with this subject of silicates, but from a very different point of view. In fact, some people do not like to hear metallurgy classed as a science; they call it an art instead. Without going into that ancient quarrel at this time, it certainly may be said that pure science has greatly profited by the work which has been done in the field of metallurgy. Metallurgy is utilitarian at foundation, without a question. It is not science for science sake, which dominates its followers. The effort of metallurgists is rather to use science so as to make it the handmaiden of industry.

The metallurgist has done much in assisting the chemist to a better understanding of silicate structure, because he has approached it from a new point of view. The mineralogist deals principally with definite crystaline or crystalizable compounds. He has been engaged in discovering just what elements the various inorganic substances, which he finds in nature, contain; and in what relationship they stand to each other. But, back of all of his study and uncertainty is the strong conviction, given him by the chemist, that his minerals are always definite structures, and that it only remains to him to secure pure specimens to find their formula becoming rational.

Not so with the metallurgist. His work lies in tearing down mineral forms, not in seeking to build them up. He knows mineralogy, it is true, because to that field he must go for his crude supplies. But to him, the mineral is interesting principally as an ore, from which he can produce a metal. He studies it to see how much metal it contains, and next with what other worthless material it is associated. To separate these two portions, by physical or chemical processes, is the work of the metallurgist.

In the commoner phases of the work, such as iron or lead smelting, it is the custom to reduce the whole body of the ores to a fiery liquid silicate, of the most fusible sort attainable with cheap and easy accessible fluxes, accompanied with such violent reducing action that the easily reducible oxides will come down to metallic form. From the fluid mass, the metal separates out by gravity, accumulating at the bottom of the fiery pool, while above it, floats the slag composed chiefly of silica and the easily reducible oxides, such as the earths, alkalies and alumina.

These fluid silicates or "slags" of the metallurgist offer, in some respects, an analogy to the fluid fiery magmas of which our earths whole crust was once formed and of which we still have experience in occasional volcanic outbreaks. They differ however in two important respects: (1) they are generally more basic than natural lavas, being compounded so as to give the greatest fusibility and fluidity at the temperature at which the smelting is to be done; and (2) their usefulness is generally complete when the metal has been reduced from them and they then are allowed to cool as fast as possible in the open air. Hence, they have small opportunity for the formation of crystaline minerals, as the real lavas do, which flow out in such enormous mass as to retain their heat for years or even centuries, and which offer every advantage for the gradual growth of crystaline forms.

Nevertheless, a number of distinct crystaline minerals have been recognized and determined in metallurgical slags; enough to show the similarity of the processes of lava and slag formation, and to illustrate the development of mineral forms from slowly cooling fluid silicates.

The slag is therefore to the metallurgist, merely a convenient and effective way of separating off the earthy or silicious gangues of his ores; and since so many and such important processes are thus executed, great study has been devoted to the principles of slag formation or the fluxing of ores. The metallurgist, all unhampered by such considerations as have influenced the mineralogist, in his studies of the relation between the chemical composition or slags and their fusibility and reducing qualities, has developed a system of calculation and comparison which offends the sense of propriety of the stickler for pure science in the chemical field. He finds that certain bases with certain acids make good fusible slags, if used within certain ratios. He finds further, that it is generally better to use several different bases to a given amount of acid, than to depend on the fluxing value of any one base alone. He finds that the easiest and most natural way to maintain these proportions is by the use of ratios, and in order to avoid the constant use of equivalent weights, when different oxides are being used, he makes his calculation on the contained oxygen of the elements.

The oxygen-ratio was not devised by metallurgists, but its convenience and value as a system of comparison and control, early commended itself to him and is now firmly engrafted in his work. He classifies silicates by their oxygen-ratio: A protosilicate is to him, one in which the oxygen from all of the bases equals that from all of the acids. In a bisilicate, the acid oxygen is double that derived from the bases.

If all metallurgical products were either proto or bi-silicates, or of some other definite ratio, the disagreement between the mineralogical and metallurgical use of the oxygen ratio would be minimized. But, slags are not made with this object in view. They are made to fuse easily, let their composition be what it may; and when duly reduced to their molecular ratios, are likely to come at some irregular fractional ratio, which is neither a proto or bi-silicate, but lying somewhere between them. The chemist, conceiving of definite compounds, dislikes to use an expression indicating a fractional molecule or an uneven ratio, as such are incompatible with the theory of chemical combination. The metallurgist, on the other hand, does not pretend that his slag is a definite chemical compound, but merely a mixture of uncrystalized, inseparable chemical compounds, chilled into the semblance of a natural mineral by sudden cooling. The ratios of the various elements which compose this once fluid mass must be considered, by all chemical evidence, to be in definite proportions in each compound they have formed, but the sum of all molecules of each kind does not by any means form a ratio which indicates chemical proportions for the whole mixture.

The oxygen ratio therefore is the easy, natural method of contrasting such compounds, regardless of its apparent inconsistency with the laws of chemical combination.

THE CERAMIC CLASSIFICATION.

A fourth branch of chemical technology has in recent years been differentiated from these others in the field of silicate study. This newest division of the subject covers our own specialty, the ceramic field. Although the latest to enter the arena, it cannot truthfully be said that it has entered with the fullest training or with the broadest realization of what has been done in other parallel lines.

The field of the ceramist, though well defined in some respects, is closely affiliated with or co-extensive with that of the metallurgist, in this matter of slag or silicate formation. The metallurgist produces ordinarily slags which are so basic that they form stony lusterless masses when cool; crystals ordinarily have no time to grow to appreciable size, but the mass is found to be an aggregation of finely divided crystaline matter when examined under the polariscope. The mineralogist calls this condition "cryptocrystaline."

The ceramist, in dealing with bodies and refractory materials, produces results very similar indeed to the above described phase of metallurgy. He does not produce these wares by fusion; he takes already crystaline minerals, blends them, and by heat merges the identity of one mineral in to another, until a mass of matter is formed, which may be either amorphous or cryptocrystaline, according to the point to which combination has been brought. The higher the burning temperature, the more perfect the vitrification, and the more likelihood of the production of crystaline compounds from the magma of partially fused matter.

But the ceramist is often most interested and concerned in the production of the glassy silicates which form the basis of nearly all of his decorative work. A glass is a substance about which even at this day much misconception exists. It is sometimes spoken of as a "solid solution." It represents an accumulation of silicate matter, whose ingredients are not present in ratios which easily form crystals. For the most part, they are too high in silica, and while a glass is not uncrystalizable, it does not flash readily into the crystaline state, as the basic slags do.

These views are much strengthened by the fact, easily proven by experiment, that a glassy silicate does form definite crystaline compounds if given a long time to cool and also that a stony silicate, if very rapidly chilled from the fused condition, by pressure between cold surfaces, is converted to the glassy state. Thus it is seen that the glassy condition is more or less an accident of cooling, and that the same substance may form cryptocrystaline slags or lavas in one case and glasses in another.

Now, in the field of the ceramist, the glassy silicates are the subject of his principal study. And they present the most difficult and most intangible field of all, from the very fact of their glassy amorphous nature. To study them at all, some method of comparison of composition is necessary as a first step. For this purpose the ceramist uses the fractional molecular formula. He does not like the oxygen ratio of the metallurgist because of its lack of particularity.

The metallurgist in making a slag, need not care particularly whether one base or another is present in the greatest proportion, or whether his slag alters considerably in composition, if it maintains the general type which his operation requires. In short, the slag is by-product, and is only designed to gather up the more or less fluctuating gangue of the ores and convert it into a silicate, which will permit the metal to separate from it and which will itself flow out freely. Sometimes, as in the basic process of steel manufacture, the slag has a distinct chemical office to perform for the metal, but this is the case oftener in refining, than in smelting operations. Consequently, rather large fluctuations in composition may occur, both in make up of the RO fluxes and the total ratios of base to acid, without detriment to the work.

But with the ceramist, who is treating the delicate problems of glaze and body, every little difference or fluctuation of any sort, counts heavily. He is striving to maintain that delicate adjustment or balance between crazing and shivering, which is the nemesis of most potters, and that which is to the metallurgist, a trifling fluctuation in a by-product, is to him a serious alteration in his principal product.

Consequently, the oxygen formula, which expresses merely the crude, gross-ratios, without specification of the details, does not suit his need, and in place of it, he used the molecules from which the oxygen is derived, each expressed in full form.

Molecular ratios and oxygen ratios are both founded on the old conception, no longer admitted by chemists, that chemical compounds are unions of molecules instead of atoms. Chemists used to write

Calcium Carbonate. CaO, CO_2.

Now, they write

$$CaCO_3$$

and they say that the previous molecules, CaO and CO_2, which were merged in the new compound, no longer exist.

The new theories of chemical combination are unquestionably far better and more far-reaching in the scope than the old ones. No one who has followed the brilliant work of the last generation can doubt this. But, the fact remains, that the old conceptions, while they are not literally true, at least offer a more tangible basis for calculation and expression of the facts in industrial work than anything which the chemists have brought out since.

A calm and dispassionate view of the facts seems to justify the use of these methods of expression, *especially in the realm of the cryptocrystaline or amorphous silicates*, on the ground of their *convenience.* No other ground should be for one moment set up. The chemists are undoubtedly right in criticizing these methods as not to be harmonized with newer and better views of chemical truth. But their newer methods of expression do not yet reach and embrace this field in which we are working, We cannot give up the present method until a better one is open.

In short, the oxygen ratio and the molecular formulæ, both involving the expression of fractional molecules, are to be viewed merely as mechanical aids in the expression of facts, otherwise inexpressible. They owe their value to their graphic character. By no other than a graphic system, can anyone attempt to marshal the intricacies of a series of glazes into a form which can be either understood or compared.

The chemists themselves use graphic methods of expression as freely as any other scientists. The famous benzene ring, which has done such wonderful service in the field of organic chemistry is a conception which could never have been developed and brought to its present security, had it not been for the hexagonal drawing by which it has been represented. This customary figure is simply a visible material aid to an inmaterial conception. Most minds need the stimulus of the concrete, in order to soar even a little way into the abstract, and it is safe to say that organic chemistry would be several decades behind its present stand, if its votaries had denied themselves the mechanical aid of such things as graphic formulæ in general.

There still remains a topic in this connection in which the ceramists have laid themselves open to just criticism from the other silicate chemists, viz: our nomenclature.

The chemist denies that a chemical compound can still be considered as a union of molecules, and objects to the molecular or oxygen ratio on this account. But, in the older mineralogical and modern metallurgical nomenclature, this objection is much weakened in its force by the fact that in all of those systems, the difference is merely one of interpretation, not of method of classification or nomenclature.

For instance, Dr. Clarke (previously quoted) states that there are five known silicic acids to which all known crystaline silicates can be referred. These he names and formulates as in column I and II of the following table.

TABLE I.

Chemical Name	Chemical Formula	Molecular Formula	Oxygen Ratio
Ortho-Silicic Acid.....	H_4SiO_4	$2 H_2O, SiO_2$	1 : 1
Meta-Silicic Acid.......	H_2SiO_3	H_2O, SiO_2	1 : 2
Tri-Silicic Acid.........	$H_4Si_3O_8$	$2 H_2O, 3 SiO_2$	1 : 3
Di-Ortho-Silicic Acid...	$H_6Si_2O_7$	$3 H_2O, 2 SiO_2$	3 : 4
Di-Meta-Silicic Acid....	$H_2Si_2O_5$	$H_2O, 2 SiO_2$	1 : 4

In columns three and four are shown the old way of viewing these compounds, which, apart from all ideas of chemical truth, has in the minds of the old school at least, a much clearer and more comprehensible appearance than the new nomenclature.

But, the oxygen ratio of the metallurgist and older mineralogist had no quarrel with the present methods in the results of its classifications. The ortho-silicate of today is the uni-silicate of yesterday. The tri-silicate of today has always been so called. The oxygen contents of the positive and negative sides to each compound, formed a reliable guide to its classification by the newer chemical basis as well. A uni-silicate by one is a uni-silicate by the other.

But, the ceramist has drifted off into the erroneous habit of naming a silicate by the number of molecules of acid and base respectively. In the following table are given four minerals, which are treated by the different methods of classification:

[See table on next page.]

From this table, it is clearly evident that the ceramic method is founded upon an inaccurate conception and ought not to stand.

In short, the method of displaying a glaze composition in RO, R_2O_3, and acid molecules is most useful to the ceramist on account of its graphic character. But it should not, as was occasionally the case in the writings of Seger and most others who have followed him in this field, be the basis of an attempt to classify or name the silicate itself. The latter must proceed from considerations of its real chemical valence, of which the quantivalent ratio of Dana or the oxygen ratio of the metallurgist are the convenient working expressions.

An example of the misconceptions, which the lack of some co-ordinating influence among these allied branches of industrial chemistry has permitted, can easily be given. The metallurgist has said for years past, that the area of most fusible slag formation lies between the proto and bi-silicates. Seger, in the ceramic field, has told us that the glasses and glazes lie between the bi- and tri-silicates. At first sight, a beautiful connection seems apparent, and it appears that one merely steps from the realm of slags directly into that of the glassy silicates, by passing from one stage of acidity to the next

But, plausible as this theory seems, many phenomena arise which do not seem to fit with it.

The timely paper of Professor Binns, calling attention to Seger's lapse in this matter, was the means of bringing out the fact that real glasses, which Seger correctly says, fall between RO, 2 SiO_2 and RO, 3 SiO_2, represent the extreme

TABLE II

COMPARISON OF SYSTEMS OF SILICATE NOMENCLATURE.

Proper Name of Mineral	Chemical Formula	Chemical Name	Mineralogical Classification	Metallurgical Classification	Ceramic Classification
			Quantivalence of Base vs Quantivalence of Acid	Oxygen in Base vs Oxygen in Acid	Molecule of Base vs Molecules of Acid
Zircon	$ZrSiO_4$	Zirconium Ortho-Silicate	Uni-Silicate of Zirconium 8 : 8	Proto-Silicate of Zirconium 1 : 1	Mono-Silicate of Zirconium ZrO_2, SiO_2 1 : 1
Willemite	Zn_2SiO_4	Zinc Ortho-Silicate	Uni-Silicate of Zinc 8 : 8	Proto-Silicate of Zinc 1 : 1	Sub-Silicate of Zinc $2\,ZnO$, SiO_2 2 : 1
Xenolite	$Al_4Si_3O_{12}$	Aluminum Ortho-Silicate	Uni-Silicate of Alumina 24 : 24	Proto-Silicate of Alumina 1 : 1	Sesqui-Silicate of Alumina $2\,Al_2O_3$, $3\,SiO_2$ 2 : 3
Wollastonite	$CaSiO_3$	Calcium Meta-Silicate	Bi-Silicate of Calcium 4 : 8	Bi-Silicate of Calcium 1 : 2	Mono-Silicate of Calcium CaO, SiO_2 1 : 1

upper range of acidity in the series of manufactured silicates, and that an intermediate group, the glazes or glassy silicates in which alumina is a constant factor, come in between the true glasses and the slags. These glazes, as was pointed out by Binns, may be of a much lower oxygen ratio than Seger assigned. We now know that they occupy intermediate ground, some of them falling as low as proto-silicates, the best falling in the region of the bi-silicates, and the harder varieties going up still to the tri-silicate ratio. Above and beyond this come the non-aluminous glasses, with oxygen ratios of 1 : 4 or even 1 : 6.

We now can see much more clearly how it is that acid slags may easily, by sudden cooling, pass into the glassy condition, and also why basic glazes, by too slow cooling, become dead or matt, by assuming the cryptocrystaline condition of slags. And in the transitional stage from the area of the glazes to that of the glasses, we can see how the acid glazes become spoiled by the crystalization of minerals from their matrix, in the slow cooling of the potter's kiln, which would be perfectly free from defect, if cooled quickly like the glasses. And, also, we see how the non-aluminous glass, with its extremely silicious nature would infallibly crystalize, if it were not cooled down below the crystalizing point in a moment after leaving the glass pot.

In summing up, it may be said:

First. That ceramists and metallurgists should unhesitatingly adopt the work and views of the modern school of chemistry, in so far as these views are used in interpreting and understanding abstract chemical principles.

Second. That the molecular formula and the oxygen ratio, though based upon a chemical conception long since abandoned, still afford the best means of studying and exhibiting the composition and properties of the cryptocrystaline and amorphous silicates, with which the methods of expression of the new school of chemists seem still inadequate to deal.

Third. That ceramists and metallurgists are justified, therefore, in the continued use of these methods of expression, until one more in consonance with existing chemical theory and equally convenient in practical use is brought forward.

Fourth. That they should at all times bear in mind the fact that these methods are merely mechanical aids to expression, and are not in themselves a correct statement of chemical fact as now understood.

Fifth. That ceramists should give up the careless custom of classifying silicates by their molecular ratios, and adopt instead, that of the oxygen ratio, which system agrees in its results, if not in its methods, with the modern school of chemists.

DISCUSSION.

Dr. W. H. Zimmer: I cannot see why the different positions of the different elements in the structure of a silicate should not have some influence on its properties, just as well as the properties of a hydro-carbon are influenced by its method of combination. If you go back fifty years, inorganic chemistry overshadowed the organic. Since that, organic chemistry has overshadowed the inorganic; and I venture to say that one cause for this is that the organic chemists have paid great attention to the grouping of the elements composing these compounds. And I expect if we pay the same attention to the grouping of our silicates, we will find the way the grouping is done has an equal influence on the general properties of our silicates.

[Dr. Zimmer then drew on the black-board the pictorial diagram of the Benzene ring, and showed how the physical properties of its derivatives depended on the position or point at which the substitution of molecules occurred, fully as much as on the nature of the molecule substituted.]

Mr. E. C. Stover: I would like to ask Prof. Orton to change a formula from a mono-silicate to a bi-silicate formula, or to illustrate the mono-silicate ratio and bi-silicate ratio.

[This was done on the blackboard.]

The Chair: My mono-silicate glazes of last year's paper were meta-silicates; one of base to two of acid, on an oxygen ratio.

Mr. Stover: In reading Professor Binn's paper, some of us did not understand it clearly.

The Chair: In the printing of my paper, an error crept into the formula, the sub-figure three being left out. It was an important point in the paper. According to the methods recommended by Professor Orton, which I am sure are desirable, my mono-silicate would be a bi-silicate.

Professor Orton: (Referring to blackboard.) PbO, 0.5 SiO_2 is a mono-silicate, on an oxygen ratio, having a ratio of one to one. Take one molecule of silica and one molecule of lead PbO, SiO_2, and we have a ratio of one to two, or a bi-silicate. Mr. Binns used an expression having this last general formula, except that his glazes contained alumina also, which made, of course, a corresponding increase in the amount of silica present. Professor Binns gave his glazes as mono-silicates from the molecular ratio; they are bi-silicates from the oxygen ratio. Professor Binns pointed out clearly last year that Seger's statement that a glaze must fall between a molecular bi- and tri-silicate was not true, at least for the class of bodies and glazes he was working upon.

The Chair: As a matter of fact, that has also been brought out in Mr. Fickes' paper, which will be read presently.

THE PERMISSIBLE VARIATION IN THE SILICA AND ALUMINA CONTENTS OF RAW LEAD GLAZES.

BY

WALTER MORGAN FICKES, E. M.

The late Dr. Seger in his article "Ueber Glasurfehler und deren Ursachen" (page 451 Seger's Gesamelten Schriften) makes the statement, without supporting it with any data whatever, that "If a glaze shows itself too fusible, it can be hardened by raising the silica content, and thereby decreasing the content of the fluxes. A limit must be observed in this respect, in that the acidity of the glaze must never reach a tri-silicate and never fall below a bi-silicate."

Professor Chas. F. Binns, of the Trenton Technical and Art School, attempted to produce glazes along these formulæ and failed. His conclusion was (see Trans. Am. Cer. Soc., Vol. II) that the trouble was due to too high an acid content, and he reduced it to what he calls a mono-silicate. In these experiments, the trials were burnt at cone 3 and 8, temperatures which are extremely high for raw lead glazes. And in subsequent work at Ohio State University, it was found that better and more consistent results could be obtained at a lower temperature (cone 02).

Prof. Binns in his summary makes the following statement "There is no doubt that each base requires its own content of alumina, and consequently of silica, but so far as we have gone, and all our ceramic research must have its limitations of application, the mono-silicate ratio cannot be successfully departed from in *raw lead* glazes." The statement of Binns and Seger in themselves are in direct opposition, and it was, therefore, undertaken to reproduce the data of Binn's work at the Ceramic Department of the Ohio State University.

The apparent differences between these authors may be entirely due to the local conditions existing at the places of experimentation, or it may be that a meaning has been taken from Seger's statement different from the one which he originally intended. It is to be greatly deplored that Seger did not give the data from which he deduced this law; in its absence we can only depend on what we know of his methods of experimentation.

At the time Dr. Seger made these experiments, the dry press process was not used to any extent in Germany, and it is probable that he used trials made by a plastic process. Now, in the experiments made by Binns, dry-pressed bodies were employed. This brings in a factor of variation, whose value and influence is as yet little known, and which has not been investigated. It seems to be the general opinion that a porous dry-press body would be much more easily attacked by a glaze than a body made from the same clay by a plastic process; for in the latter case, the body is more compact and closer-grained than in the case of a dry-press body. (See remarks by Mayer and Walker, Trans. Am. Cer. Soc., Vol. II.)

A second factor of great importance is the nature of the body and glaze. If, for example, Seger referred to fritted boro-silicate glazes, his assumption is right, for this type of glaze has a much higher content of acid than the raw lead glazes. Here again we are handicapped by the lack of information regarding Seger's experiments. If we assume that the temperatures employed by Seger were such as are ordinarily used for lead glazes (cone No. 05 to No. 1), it is quite evident that we should expect some differences in the results of Seger and Binns, for the latter conducted his work at from cone 3 to 8.

And again, it may be that Seger referred to the composition of the glaze after burning, and not to the raw mixture. This is possible, but not probable.

It may, therefore, be concluded that the differences between the statements of these two writers may be due to variation in conditions which they have not stated, or their work may have been directed toward entirely different portions of the great field of lead glazes.

The general problem which the work of these two preceding investigators suggests is the determination of the limits of composition, within which good raw lead glazes may be produced. Every one knows that if too much silica be added to a glaze, it becomes crystaline, and useless as a glaze. The question is, how much? Seger says between a bi-silicate and a tri-silicate. Binns says not only that a mono-silicate will work, but also that its ratio cannot be departed from if the best results are to be obtained.

In settling this matter of how acid a glaze may be, we at once come into contact with another fact, viz: That it will depend on how much alumina is present with the silica.

Every one knows that alumina tends to prevent the devitrification of a glaze, and also to control its fusibility. Hence, we see that more silica may be used, if more alumina is also used to offset it, but that the resultant mixture must have more heat to make it mature as a glaze. So we see that the field opening up is so extensive and the factors causing variation are so numerous and important, that an examination of the whole field is impossible under present limitations. Therefore, we can only hope to accomplish definite results in this thesis, by restricting ourselves to a very small portion of the whole problem.

FACTORS OF VARIATION

If we would undertake to investigate the entire field, the following factors of variation would have to be considered:

First. Variation in nature and amount of the substances making up the RO elements. For example, taking lead and lime as the base forming materials, we might have all combinations from no lime up to all lime. This same would be true of any two materials, such as soda, potash, lead, baryta, magnesia, etc. In addition to all these combinations, we would also have an almost infinite number of other combinations by using three or more substances. Then again, any change made in the alumina and silica would necessitate a duplication for each of the above combinations. It is, therefore, evident that this investigation must be restricted to a very few simple combinations.

Second. Variation in relation of parts.

(*a*) Variation of oxygen ratio.

By the oxygen ratio is meant the ratio between the oxygen of the base forming elements *i. e.* (RO and R_2O_3), and the oxygen of the acid forming elements, (SiO_2 and B_2O_3).

For example:

$$1.0\ RO,\ 0.15\ Al_2O_3,\ 1.50\ SiO_2$$

$$\left.\begin{array}{ll} \text{Oxygen in RO} & = 1.00 \times 1 \text{ part } O = 1.00 \\ \text{Oxygen in } Al_2O_3 & = 0.15 \times 3 \text{ parts } O = 0.45 \end{array}\right\} = 1.45$$

$$\text{Oxygen in } SiO_2 = 1.50 \times 2 \text{ parts } O = 3.00$$

$$\text{The Oxygen Ratio is } \frac{3.00}{1.45} \text{ or } \frac{2.07}{1} \text{ or } 1 : 2.07$$

The oxygen ratio of a glaze can be made to vary by varying either the silica or alumina or RO elements.

(*b*) Variation in the molecular ratio of the alumina to the silica.

By this is meant the ratio between the number of molecules of alumina and the number of molecules of silica. For instance, take the glaze

$$\left.\begin{array}{l} 0.7\ PbO \\ 0.3\ CaO \end{array}\right\} 0.2\ Al_2O_3,\ 2.00\ SiO_2$$

Here, there are 0.2 molecules Al_2O_3, to every 2.00 molecules of SiO_2, therefore the ratio is 0.2 : 2.0 or 1 : 10.

This molecular ratio plays an important part in the control of a glaze, since by it the hardness, tendency to flow or devitrify, etc., are influenced.

Third. Variation in burning temperature.

It is known that lead glazes can be produced at temperatures ranging from cone 010 to cone 8, and, of course, the composition of the glaze maturing at either of these temperatures, or at some intermediate temperature, would vary considerably. Hence it would be necessary to restrict ourselves to one temperature in order that the work should be comparable throughout.

Fourth. Variation of kiln conditions.

(*a*) Mode of setting.

The manner in which the ware is placed in the kiln, whether close together or with large open spaces between, exercises an important bearing upon the degree of perfection attained. If the ware is close set, there is not enough draft space, and the heat not being carried away from the fire box causes intense local temperature, but the ware is underburnt. If, on the other hand, the ware is set too open, the draft finds little or no resistance and carries the heat through the kiln, without giving time for the ware to absorb it. Hence, if we vary the manner of setting radically from burn to burn, we bring in a factor of variation of great importance.

(*b*) Variation in firing conditions.

The influence of kiln gases through oxidation and reduction is quite well known, and needs no explanation. Under this head would also fall—amount of draft, kind of fuel, thickness of bed of fuel on grate, etc.

(*c*) Maturing period.

It is a well known fact that substances when exposed to a long continued heat will melt at a lower temperature than if subjected to a greater temperature for a short period only. This is especially true of glasses and glazes, and its effect would be quite an important factor, and must be regulated to some standard condition.

(*d*) Variation in cooling conditions.

Previous work in this department has shown that if the kiln were closed tight and a heavy bed of fuel left on the grate bars, the glazes would come out badly scummed by sulphur compounds. But if cooled in a clean pure atmosphere, the same glazes would be good.

This partial list of the factors which may influence the results of an experiment, will suffice to show that the conclusions of no experimenter in this field can be accepted at face value, unless accompanied with a carefully drawn statement showing every condition by which they were influenced. We believe that the differences already quoted would not be found if such statements had been made.

LIMITS ASSUMED IN PRESENT WORK

It is now necessary to indicate just what portion of this great field of investigation the present experiments will touch upon.

The real aim of the work is, to define what range of composition a raw lead glaze may have, as to its silica and alumina contents, and still be a workable glaze. And to narrow the inquiry down, so that it admits of completion in the time available, the following restrictions will be made:

First. RO elements.

We will restrict ourselves to the use of two combinations of RO elements. 1st, (0.7 PbO, 0.3 CaO). This was chosen partly because it was the RO used by Binns in his work, and partly because it is the extreme amount of lime which can be taken into solution readily, in a glaze whose other flux is PbO alone, at the temperature of 02. This produces relatively a hard and easily crystalized glaze. 2nd, (PbO). This was chosen because it is the opposite extreme, *i. e.* it produces the softest and most fusible of raw lead glazes.

Second. Ratios of the glaze.

(*a*) The oxygen ratio was selected as the best basis of comparison of glazes; Seger's bi-silicate and tri-silicate become, when measured by the oxygen ratio, 1 : 4 and 1 : 6 respectively. Prof. Binns' work showed clearly that these ratios are impossible in the class of glazes which he treated and with the conditions under which he worked. A little preliminary work brought us to the same conclusion. On the other hand, Prof. Binns recommends the bi-silicate (oxygen ratio) as the best glaze. We determined to begin with the ratio 1 : 3, which we felt sure was too acid, and come down by stages 1 : 2.5, 1 : 2, 1 : 1.5, to 1 : 1, which latter we felt sure would be too basic for a good glaze.

These ratios were made the basis of separation of our glazes into batches or series, one being made for each ratio and with each RO mixture employed, making ten in all.

(*b*) The molecular ratios.

Seger recommends that the alumina shall be about one-tenth of the silica in an ordinary glaze, *i. e.*, the standard molecular ratio is one to ten. In this series of tests, one object was to determine for ourselves the truth of this proposition and accordingly, the glazes we planned, were made to vary between an alumina-silica ratio of 1 : 5 on one end of the series up to 1 : 25 on the other. In the first, the glaze was very much higher in Al_2O_3 than any usual glaze and in the other extreme, the alumina became so small as to exercise little influence.

Third. A standard temperature of cone 02 was adopted, as it is the temperature at which the largest number of all lead glazes are required to mature in commercial practice.

Fourth. The kiln conditions.

(*a*) Setting.

(*b*) Firing conditions.

These conditions will be discussed fully under head of Burning.

(*c*) Maturing period.

The kiln was maintained at about the finishing temperature (cone 04–02) for a period of ten hours. This compares favorably with the length of time given in actual kiln work.

(*d*) The ware was allowed to cool in pure atmosphere, free from sulphur gases, etc., by drawing fire and ashes at end of the ten hours' maturing period, and allowing the ware to cool with stack damper wide open.

THE PLANNING OF THE EXPERIMENTAL GLAZES.

To accomplish the purposes set forth in the foregoing, five series of glazes were designed. Each series maintained one oxygen ratio between its total basic elements and its total acid elements, but its proportions of alumina and silica were constantly so changed as to produce successively a series of different molecular ratios. Series No. 43, given on the next page, illustrates the method.

These glazes were applied to small dry-pressed tiles, made from bodies representing the three general types of clay wares, *i. e.*, red, buff and white burning bodies.

From a study of this table, it will be seen that all of the glazes given are extremely soft glazes, and that none of them come within the usual range of manufacturing conditions except possibly the last one or two. However, the acidity of the series is very low, and it was thought necessary to make the glazes cover not only the field of usage, but the field of practicable limits as well.

SERIES No. 43.

Numbers of Glazes.	COMPOSITION OF GLAZES.				Oxygen Ratio. Total Base to Total Acid	Molecular Ratio. Alumina to Silica.
	PbO	CaO	Al_2O_3	SiO_2		
443	0.7	0.3	0.033	0.824	1 : 1.5	1 : 25
442	0.7	0.3	0.043	0.846	1 : 1.5	1 : 20
292	0.7	0.3	0.055	0.875	1 : 1.5	1 : 16
293	0.7	0.3	0.064	0.894	1 : 1.5	1 : 14
294	0.7	0.3	0.076	0.920	1 : 1.5	1 : 12
295	0.7	0.3	0.097	0.970	1 : 1.5	1 : 10
296	0.7	0.3	0.110	1.000	1 : 1.5	1 : 9
297A	0.7	0.3	0.130	1.042	1 : 1.5	1 : 8
297B	0.7	0.3	0.160	1.110	1 : 1.5	1 : 7
298	0.7	0.3	0.200	1.200	1 : 1.5	1 : 6

Five such series of glazes were made, having successively a total acidity of 1 : 1, 1 : 1.5, 1 : 2, 1 : 2.5 and 1 : 3, in which the RO elements were 0.7 PbO, 0.3 CaO and a similar five series in which the only difference lay in the RO, which consisted of PbO alone.

MANUFACTURE.

The extreme members of each series were weighed up, ground wet in a porcelain lined jar mill (9¾″ diameter x 12½″ deep, inside) with a charge of flint pebbles, for one and one-half hours. When ground, the resulting slip was screened through a 150 mesh brass screen and allowed to settle, and the supernatant liquid removed. Moisture determinations were then made, and the amount of dry material per gram of fluid glaze was ascertained. With these data, it was easy to calculate the intermediary members of the different series, for, having the same RO and the same ratio between oxygen of base and oxygen of acid in both extremes, there was only one variable—the relation of Al_2O_3 to SiO_2, which was calculated as follows:

Take, for example, Series No. 43, just given, in which the extremes are No. 298, having the formula,

$$1\ RO,\ 0.2\ Al_2O_3,\ 1.20\ SiO_2$$

and No. 443, having the formula,

$$1\ RO,\ 0.033\ Al_2O_3,\ 0.824\ SiO_2$$

To calculate the amounts of each of these extremes entering into No. 295, whose composition we wish to make

$$1\ RO,\ 0.097\ Al_2O_3,\ 0.970\ SiO_2$$

we proceed as follows:

0.200 Al_2O_3 in No. 298
0.033 Al_2O_3 in No. 443
0.167=difference between extremes.

0.200 Al_2O_3 in No. 298
0.097 Al_2O_3 in No. 295
0.103=difference between 1st extreme and No. 295.

$$\left.\begin{array}{r}\frac{0.103}{0.167} = 0.6173 \text{ of No. 443}\\ \text{and } (1.-.6173) \text{ or } 0.3827 \text{ of No. 298}\end{array}\right\} \text{ to make 1.0 of No. 295}$$

That is, by dividing the difference between the two extremes into the difference between the first extreme and the glaze to be made, we obtain the (decimal) amount of the second extreme. This method is repeated for each of the other intermediates. The strength of the slips must, of course, be taken into account in calculating the amount of slip of each extreme to be used in making the blend.

After blending, the glazes were screened again through 150 mesh screen to insure perfect and complete mixture. Just before dipping, the glazes were again screened in order to prevent the settling and separation of the heavier materials.

Each glaze was dipped on three types of bodies: (1) White, (2) Red and (3) Buff. These bodies were in the form of tile ($4\frac{1}{4}$ inch by $1\frac{1}{16}$ inch biscuit) made by U. S. Encaustic Tile Co., and represent the three great classes of common clay wares, upon which lead glazes are used. The buff body corresponds to yellow ware, buff brick, terra-cotta, etc. The red body is similar to that used for red brick and terra-cotta, tile, flower-pots, etc. The white body is like that used for white-ware, white wall-tile, etc.

Three pieces of each color of body were used for each number; one trial being placed in the bottom third of the kiln; one in the middle third; and one in the top third. This was done in order to be certain that each glaze had an equal chance to mature in the kiln.

There was a considerable difference in the hardness of the three different bodies; the white being soft and porous; the red porous, but not very soft; and the yellow hard and less porous yet.

BURNING.

When the trials were dipped, they were placed in shallow tile setters, 8 inches by 8 inches square and 1¾ inches high. Each setter, when resting on its next neighbor, inclosed a space about 7 inches by 7 inches and 1⅜ inches high, in which a 6x6 square tile, or 7 or 8 pieces such as were used in these experiments could be placed. These setters were piled up in stacks as high as necessary and made an excellent arrangement for this work; they were carefully luted before placing in the kiln.

In the first burn the ware was placed in the kiln at the same time with some ware made by Messrs. Cannon and Williams, the whole being piled into six bungs. These bungs practically filled the entire kiln chamber, and left only enough space for draft. This burn produced mature ware.

In a burn immediately following this, and made by Messrs. Cannon and Williams alone, the ware was placed in two bungs with at least six inches open space on each side of bung—other conditions remaining the same. In this burn, the ware was immature. It was suggested that this was due to the fact that the draft carried the heat rapidly through the kiln and did not afford sufficient time for this heat to be absorbed by the ware. Therefore, it was decided to fill up the open spaces with fire-clay bats, broken setters, etc. This plan is followed in burn Nos. 2 and 3, and gave good mature ware, showing that the suggestion made above was correct.

In burns Nos. 1 and 3, coal and coke were used. It was found after burn No. 1, that many of the glazes were full of crystaline matter, and it was decided to investigate this and see if this was due to absorption of sulphur from kiln gases. To this end, burn No. 2 was made with wood and charcoal. The wood was all dry hickory, ash and hardwoods, being scrap and refuse from the Columbus buggy factory. The other conditions were held as before, and the glazes were practically the same, proving that the scum was due to the materials of the glaze segregating out or crystalizing.

The influence of oxidizing and reducing conditions is quite marked on lead glazes, and, therefore, it was decided to keep the kiln atmosphere as nearly oxidizing as possible. In burn No. 1, during the last half hour of the maturing, a sharp reducing fire was kept up in hopes of removing any sulphur absorbed by the glazes, and thereby to free them from scum. This, so far as could be observed, had no effect on the scum, and interfered with other white glazes in tne same kiln. In burns Nos. 2 and 3, this was avoided, and equally good results were obtained.

THE COOLING OF THE WARE.

The method of cooling is of almost equal importance with the burning, for previous work at O. S. U. ceramics department has showed that good glazes can be ruined if not properly cooled. In one case, a kiln was cooled with stack damper shut and fire box full of coals and ashes. The ware came out very badly scummed, and could hardly be called a glaze. When these same glazes were burnt at the same temperatures and cooled with the damper open, and all ashes and clinkers removed from ash pit, they gave good, clean surfaces as bright as a mirror. Therefore, the last conditions were adopted for these experiments.

We come now to the consideration of the specific glazes made, and the results of their burning. In order to present the facts in the smallest space, the following tabular arrangement has been devised. The number and formula of each glaze made, its oxygen ratio and its molecular ratio, are given in the left-hand columns. In the right-hand columns, the general character of the glaze is indicated by the words, "good, fair or bad." "Fair" indicates a glaze which has a few imperfections, crystaline separations or other defects, but which is so nearly perfect as to lead to the belief that it is a workable mixture if applied to a body suited to it.

SERIES No. 46.

Oxygen Ratio } 1 : 3

RO Fluxes { 0.7 PbO, 0.3 CaO

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing, Ten Hours at Cone 04–02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
287	0.130	2.085	1 : 16	bad	bad	bad
288	0.156	2.202	1 : 14	bad	bad	bad
277	0.200	2.400	1 : 12	bad	bad	bad
289	0.220	2.720	1 : 10	bad	bad	bad
290	0.340	3.030	1 : 9	bad	bad	bad
291	0.420	3.300	1 : 8	bad	bad	bad

REMARKS.

Every glaze in this series failed to produce a glaze or even a glassy mass. The surface of all of the trial pieces, red, buff or white, had the appearance of white frosting. This proves that for these three bodies, at this temperature and with this RO combination, the oxygen ratio of 1 : 3 is not available, at any molecular ratio of alumina to silica. The molecular ratios 1 : 9 and 1 : 8 were decidedly more glassy and better fused than the others. This indicates that the lack of glassy structure in the others is due to devitrification, and that the high alumina contents was able to partly hold this tendency in check, while the low alumina glazes were not.

SERIES No. 45.

Oxygen Ratio } 1 : 2.5

RO Fluxes { 0.7 Pb O / 0.3 Ca O

Glaze Number.	Proportions of Alumina and Silica to 1 RO		Molecular Ratio between Alumina and Silica	Character of Glazes after Firing Ten hours at Cones 04-02.		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
246	0.100	1.625	1 : 16	bad	bad	bad
283	0.125	1.720	1 : 14	bad	bad	bad
247	0.150	1.812	1 : 12	bad	bad	bad
299	0.200	2.000	1 : 10	bad	bad	bad
284	0.230	2.110	1 : 9	bad	bad	bad
285	0.300	2.370	1 : 8	bad	bad	bad
285A	0.390	2.712	1 : 7	better	better	bad
286	0.500	3.125	1 : 6	better	better	better

REMARKS.

White Body.—Numbers 246 to 285 are glassy, fairly smooth silicates, full of suspended crystaline segregations. Number 285A is nearly free from crystalization, but have small bare areas due to the glaze not running well, being too refractory for the heat used. No. 286 shows the same defect more pronouncedly. Both of these show practically no crystalization.

Red Body.—The glazes were practically the same as on the white. No good glazes, showing any promise of being workable at this temperature were found.

Buff Body.—The glazes were decidedly better on this body, than on the red or white. This improvement is probably due to the greater hardness of the buff tiles, causing them to be less attacked by the glazes and hence, making the latter less likely to segregate crystaline matter. The glazes produced between molecular ratio 1 : 16 and 1 : 8 are about equal in perfection of finish, but none are workable.

This shows that under the conditions used in this test. the oxygen ratio 1 : 2.5 is still too high for practical use.

SERIES 44.

Oxygen Ratio } 1 : 2

RO Fluxes { 0.7 PbO
0.3 CaO

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing Ten Hours at Cone 04-02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
450	0.045	1.132	1 : 25	fair	fair	fair
449	0.059	1.177	1 : 20	good	fair	good
278	0.077	1.230	1 : 16	good	good	good
279	0.090	1.270	1 : 14	good	good	good
242	0.125	1.375	1 : 12	good	good	good
280	0.140	1.420	1 : 10	good	good	good
281	0.170	1.510	1 : 9	good	good	good
245	0.200	1.600	1 : 8	good	good	good
245A	0.250	1.750	1 : 7	fair	fair	fair
282	0.330	2.000	1 : 6	bad	bad	bad

REMARKS.

White Body.—Glazes 450 and 449 are fair in quality, but are marred by several patches of crystaline scum. No. 450 worse than 449. Nos. 278 to 245 are good clear bright glazes, any of them workable at this temperature. Nos. 245A and 282 are neither of them mature, though 245A is a smoother better glaze than the best of the series 45.

Red Body.—In general, the glazes are the same as in the white body, but are less troubled with crystaline scum.

Buff Body.—Badly crazed throughout. Except for this defect (which has nothing to do with the scope of this inquiry) the glazes from 1 : 8 up to 1 : 25 are all good. The two lower members, 1 : 6 and 1 : 7 are immature.

The working range of glazes having this oxygen ratio lies, under these conditions, between a molecular ratio of 1 : 8 and 1 : 16.

SERIES No. 43.

Oxygen Ratio 1 : 1.5

RO Fluxes 0.7 PbO, 0.3 CaO

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glaze after Firing Ten hours at Cones 04:02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
443	0.033	0.824	1 : 25	good	fair	good
442	0.043	0.846	1 : 20	good	fair	good
292	0.055	0.875	1 : 16	good	fair	good
293	0.064	0.894	1 : 14	good	fair	good
294	0.076	0.920	1 : 12	good	good	good
295	0.097	0.970	1 : 10	good	good	good
296	0.110	1.000	1 : 9	good	good	good
297A	0.130	1.042	1 : 8	good	good	good
297B	0.160	1.100	1 : 7	good	good	good
298	0.200	1.200	1 : 6	bad	bad	bad

REMARKS.

White Body.—Numbers 443 and 442 have small globular, greasy-looking drops floating in the glaze and a little crystaline scum. 442 better than 443. No. 292 similar to 443. No. 293 and 294 growing better. Drop-like appearance and scum diminishing. No. 295, 296, 297A and 297B are all good, brilliant glazes. No. 298 is immature at this temperature.

Red Body.—The crystaline scum appears sooner than on the white, appearing in 295.

Buff Body.—All good glazes, except 298, which is slightly immature and badly crazed.

In this series, glaze numbers 442 and 443 were not made in the same blends as the lower part of series, but were weighed and ground separately. This accounts in part for any irregularities shown by them.

SERIES 47.

Oxygen Ratio 1 : 1

RO Fluxes { 0.7 PbO, 0.3 CaO

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing, Ten Hours at Cones 04–02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
432	0.021	0.531	1 : 25	bad	bad	bad
433	0.026	0.539	1 : 20	good	bad	bad
434	0.037	0.555	1 : 15	good	good	bad
435	0.048	0.572	1 : 12	good	good	bad
436	0.059	0.588	1 : 10	good	good	bad
437	0.067	0.600	1 : 9	fair	good	bad
438	0.076	0.614	1 : 8	fair	good	bad
439	0.092	0.640	1 : 7	bad	good	bad
440	0.111	0.666	1 : 6	bad	good	bad
441	0.143	0.714	1 : 5	bad	bad	bad

REMARKS.

White Body.—Number 432 ran smooth but was full of crystaline matter in large patches. 433 to 436 were all good clear brilliant glazes. 437 was glassy, but full of segregated matter. 438 was similar to 437 but worse. 439 immature, somewhat rough. 440 rougher and less mature. 441 not glassy at all.

Red Body.—Number 432 badly scummed with crystaline matter. 433 smooth, with smaller patches of scum. 434, ditto. 435, some patches of crystaline matter. 436–440, good, brilliant glazes.

Buff Body.—Not a clear glaze in the lot. 433 and 434 are the best.

In this series, there is a considerable variation in the value of the glazes, depending on the body used. Thus we find on the very soft, porous white body, a tendency for those glazes having a high alumina-silica ratio, to take up alumina from the body, and to form good glazes; while those of lower alumina silica ratio produce scummy glazes. On the other hand, on the harder red body, it is the glazes of low molecular ratio which are best. And again, on the hard buff body, the glazes attack the body very little, and are unable to form combinations by solution, and hence show the effect of their too great basicity.

SERIES No. 48.

Glaze Number	Composition of Glazes by Molecular Formulæ				Oxygen Ratio Base to Acid	Molecular Ratio Alumina to Silica	Character of Glaze after Firing, Ten Hours at Cones 04-02		
	PbO	CaO	Al_2O_3	SiO_2			White Body	Red Body	Buff Body
448	0.7	0.3	0	0.40	1 : 0.8	1 : ∞	good	bad	bad
447	0.7	0.3	0	0.30	1 : 0.6	1 : ∞	good	bad	bad
446	0.7	0.3	0	0.20	1 : 0.4	1 : ∞	bad	good	bad
445	0.7	0.3	0	0.10	1 : 0.2	1 : ∞	bad	good	bad
444	0.7	0.3	0	0	1 . 0	1 : ∞	good	good	good

REMARKS.

This series was produced after the others, owing to the results developed in series 47, in which it was clearly demonstrated that some glazes with as low an oxygen ratio as 1 : 1 were still good, and therefore that the bottom limit of basicity had not yet been reached.

White Body—448, good glaze. 447, good glaze, though with a peculiar dull appearance. 446, no glaze, full of segregated matter. 445, no glaze, is a vitreous mass. 444, a beautiful bright glaze.

Red Body.—448, good glassy surface but large patches of crystaline matter. 447, good glaze, small patches of scum. 446, 445 and 444 good clear glazes.

Buff Body.—448 is a crystaline mass, not matured as a glaze. 447 the same. 446, is a vitreous mass, not glassy. 445 same, curling away from edges. 444, a good clear brilliant glaze.

The results of this series are entirely anomalous and no explanation or theory can be advanced concerning their erratic behavior. The glazes on the red body seem to show that the RO base can, on a porous open-grained dry-pressed body, take up enough alumina and silica to make a glaze by itself. However, the data are too contradictory and inconsistent to attempt seriously to explain them.

SERIES 49.

Oxygen Ratio } 1 : 3

RO Fluxes } PbO alone

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing, Ten Hours at Cones 04–02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
478	0.130	2.085	1 : 16	bad	bad	bad
479	0.156	2.202	1 : 14	bad	bad	bad
480	0.200	2.400	1 : 12	bad	bad	bad
481	0.270	2.720	1 : 10	bad	best of series	bad
482	0.340	3.030	1 : 9	bad		bad
483	0.420	3.300	1 : 8	bad	bad	bad

REMARKS.

White Body.—478, material infused along edges; much crystaline matter. 479 better, still crystaline. 480, best glaze of the series, but too much crystaline matter for use. 481, less crystaline, but shows tendency to curl up at edges of cracks in the glaze while green, and these cracks form bare spots after firing. 482, bare spots due to curling. 483, bad bare spots; curling.

Red Body.—General appearance of glazes similar to those on the white body, but smoother and with fewer bare spots. 481 and 482 are the best, but contain too much crystaline matter to permit them to be used.

Buff Body.—478 and 479 show crystaline scum around their edges. 480 shows much undissolved matter, but is better. 481 is the best of the series, having run smooth and containing comparatively little suspended undissolved matter. 482, fairly good glaze but wavy, due to immaturity. 483, enamel-like, wavy.

No good or really workable glazes can be produced with as high an acidity as 1 : 3 oxygen ratio, under these conditions.

SERIES No. 50.

Oxygen Ratio } 1 : 2.5

RO Fluxes } PbO alone

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glaze, after Firing Ten Hours at Cones 04-02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
499	0.043	1.410	1 : 33	bad	bad	bad
508	0.059	1.470	1 : 25	good	good	fair
509	0.075	1.510	1 : 20	good	good	good
470	0.100	1.625	1 : 16	good	good	good
471	0.125	1.720	1 : 14	good	good	good
472	0.150	1.812	1 : 12	good	good	good
473	0.200	2.000	1 : 10	good	good	good
474	0.230	2.111	1 : 9	good	fair	good
475	0.300	2.370	1 : 8	good	bad	good
476	0.390	2.712	1 : 7	bad	bad	bad
477	0.500	3.120	1 : 6	bad	bad	bad

REMARKS.

White Body.—499, full of crystaline matter, edges dry and crystaline. 508 to 475, good clear brilliant glazes. 476 glassy but shows bare spots, due to curling. 477, semi-glassy with large bare spots.

Red Body.—499, similar to same glaze on the white body. 508 to 473 inclusive, are good clear glazes. 474 begins to show crystaline matter. 475 crystaline separations become quite prominent. 476, immature. 477, vitreous, large bare spots.

Buff Body.—Similar to same series on white body. 508 shows a tendency to immaturity.

SERIES 51.

Oxygen Ratio 1 : 2

RO Fluxes PbO alone

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing Ten Hours at Cones 04-02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
510	0.000	1.00	1 : ∞	fair	fair	fair
511	0.021	1.063	1 : 50	good	good	good
512	0.033	1.099	1 : 33	good	good	good
461	0.045	1.133	1 : 25	good	good	good
462	0.059	1.177	1 : 20	good	good	good
463	0.095	1.285	1 : 15	good	good	good
464	0.125	1.375	1 : 12	good	good	good
465	0.140	1.420	1 : 10	good	good	good
466	0.170	1.510	1 : 9	good	good	good
467	0.200	1.600	1 : 8	good	good	good
468	0.250	1.750	1 : 7	good	good	good
469	0.330	2.000	1 : 6	bad	bad	bad

REMARKS.

In this series, the glazes with exception of the two extremes, are all good clear glazes on all bodies. The 510 glazes show a tendency to immaturity on the white and yellow bodies. On the red, it shows a little crystaline matter. The aluminous extreme, 469 is not mature on any body, and shows much undissolved matter.

SERIES 52.

Oxygen Ratio 1 : 1.5

RO Fluxes PbO alone

Glaze Number	Proportion of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glazes after Firing Ten Hours at Cones 04–02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
513	0.00	0.750	1 : ∞	good	good	good
514	0.015	0.780	1 : 50	good	good	good
515	0.024	0.804	1 : 33	good	good	good
451	0.033	0.824	1 : 25	good	good	good
452	0.043	0.846	1 : 20	good	good	good
453	0.055	0.875	1 : 16	good	good	good
454	0.064	0.894	1 : 14	good	good	good
455	9.076	0.920	1 : 12	good	good	good
456	0.097	0.970	1 : 10	good	good	good
457	0.110	1.000	1 : 9	good	good	good
458	0.135	1.098	1 : 8	good	good	good
459	0.160	1.110	1 : 7	good	good	good
460	0.200	1.200	1 : 6	good	good	good
516	0.241	1.292	1 : 5	good	good	bad
517	0.410	1.670	1 : 4	bad	bad	bad

REMARKS.

All the members of this series produce good glazes, except the two aluminous extremes, 516 and 517. 516 is good on the white and red bodies, but on the buff body the glaze is badly scummed with segregated matter. 517 is immature on all bodies.

In this series, as in the preceding, it is clearly shown that the glazes with very low molecular ratio (*i. e.* relatively high in Al_2O_3) are unable to carry the alumina, and it crystalizes out.

SERIES No. 53.

Oxygen Ratio } 1 : 1

RO Fluxes } PbO alone

Glaze Number	Proportions of Alumina and Silica to 1 RO		Molecular Ratio of Alumina to Silica	Character of Glaze, after Firing for Ten Hours at Cones 04-02		
	Al_2O_3	SiO_2		White Body	Red Body	Buff Body
484	0.021	0.531	1 : 25	bad	fair	bad
485	0.026	0.539	1 : 20	good	good	bad
486	0.037	0.555	1 : 15	good	good	bad
487	0.048	0.572	1 : 12	good	good	bad
488	0.059	0.588	1 : 10	good	good	bad
489	0.067	0.600	1 : 9	good	good	bad
490	0.076	0.614	1 : 8	good	good	bad
491	0.092	0.640	1 : 7	good	good	bad
492	0.111	0.666	1 : 6	good	good	bad
493	0.143	0.714	1 : 5	good	good	bad

REMARKS.

White Body.—All the glazes of this series are good on this body, except 484, which is full of fine crystaline matter.

Red Body.—All glazes are good, though both extremes show they are at the limits.

Buff Body.—None of these glazes are good, being full of separations. The glazes have been unable to take up alumina or silica from the hard buff body and have therefore shown out in their true character as matt glazes, due to basicity.

On the white and red bodies these glazes made very fusible compounds which ran off the surface and stuck the tiles fast to the setters.

SERIES 54.

Glaze Number	Composition of Glazes by Molecular Formulæ				Oxygen Ratio Base to Acid	Molecular Ratio Alumina to Silica	Character of Glaze after Firing Ten Hours at Cones 04–02		
	PbO	CaO	Al_2O_3	SiO_2			White Body	Red Body	Buff Body
498	1.0	0	0	0.4	1 : 0.8	1 : ∞	good	good	bad
497	1.0	0	0	0.3	1 : 0.6	1 : ∞	good	good	bad
496	1.0	0	0	0.2	1 : 0.4	1 : ∞	fair	good	bad
495	1.0	0	0	0.1	1 : 0.2	1 : ∞	bad	good	bad
494	1.0	0	0	0	1 : 0	1 : ∞	bad	good	bad

REMARKS.

White Body.—494, pure lead oxide, did not make a glaze on the white tiles. It formed a dry crystaline mass. 495, with one-tenth of silica, shows a marked improvement, but is still crystaline. 496 is much better, showing little crystaline matter. 497 and 498 are both good, perfect glazes.

Red Body.—All glazes show smooth glossy surfaces, but with low silica, or none, the glaze has attacked the body vigorously, as was shown by the dark spots on the surface, where the glaze was of unusual depth.

Buff Body.—The 494 glaze is a dry vitreous mass. 495, 496 and 497 are all dry crystaline masses, showing an entirely dead or matt surface, with no glassy spots. No. 498 showed some glassy spots, but was mostly covered with segregated matter.

Series 54 is much less inconsistent and contradictory than the other special series, No. 48. It apparently shows that on a hard body, like the buff, a glaze becomes matt through excess of basic matter. On a soft body, like the white tile body, the glaze acts so energetically to supply its silica and alumina from the body, that it is only when practically pure flux is used that the results become valueless. On the red body, not only is silica and alumina taken up, but also the red clay seems to make a better supply of these substances than the more refractory white body, and the glazes are good when nothing but basic matter is supplied, and the silica and alumina all come from the body.

DISCUSSION OF THE RESULTS.

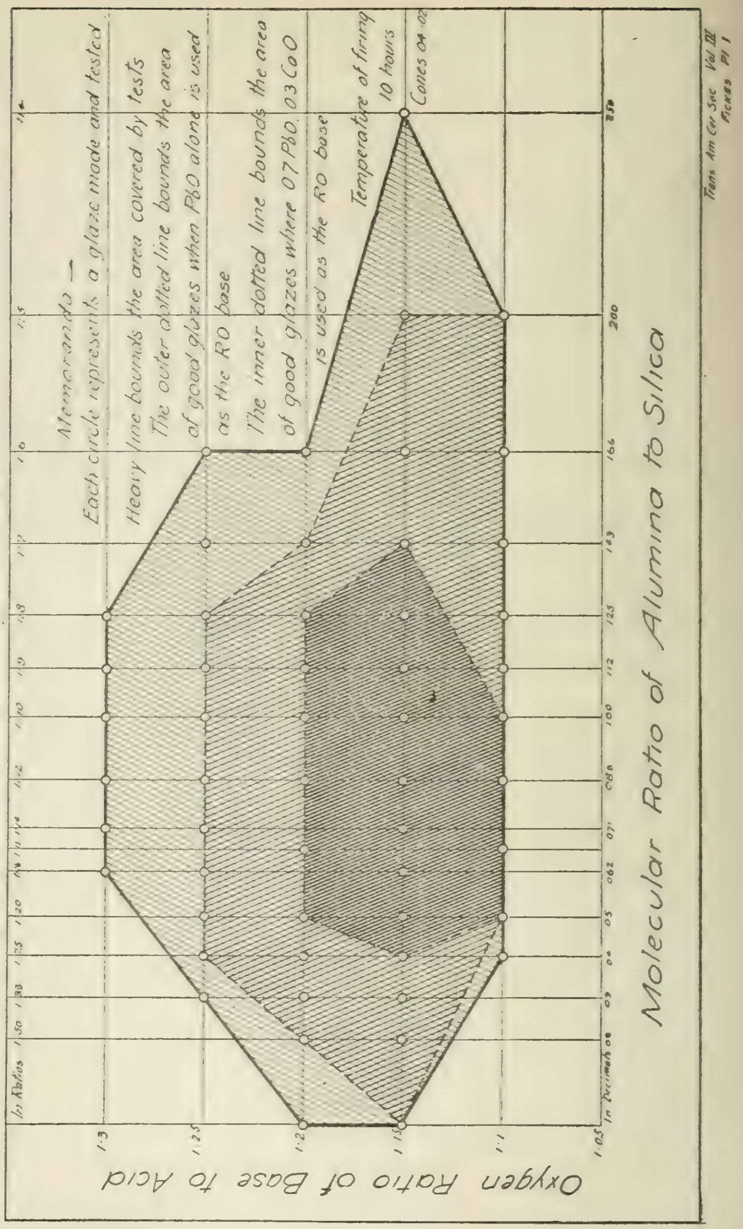
The great mass of data exhibited in the twelve preceding tables do not readily yield the underlying principles of which we are in search, without some more graphic and condensed method of expressing the facts themselves. The following plates, one for each kind of body on which the experiments were made, are designed to so assemble the data as to make their meaning clear and unmistakable to any one.

The charts are drawn so that the abscissae represent the five different oxygen ratios on which these glazes were compounded, 1 : 1, 1 : 1.5, 1 : 2, 1 : 2.5 and 1 : 3, while the ordinates represent the various ratios which the molecules of alumina bore to those of silica, running from 1 : ∞ in glazes 510, 513 and others down to 1 : 4 in the case of glazes 517.

The position of every one of the 108 different glazes made and tested can thus be located on the chart by means of circles. A line drawn around the outside members of these series marks the area of the test.

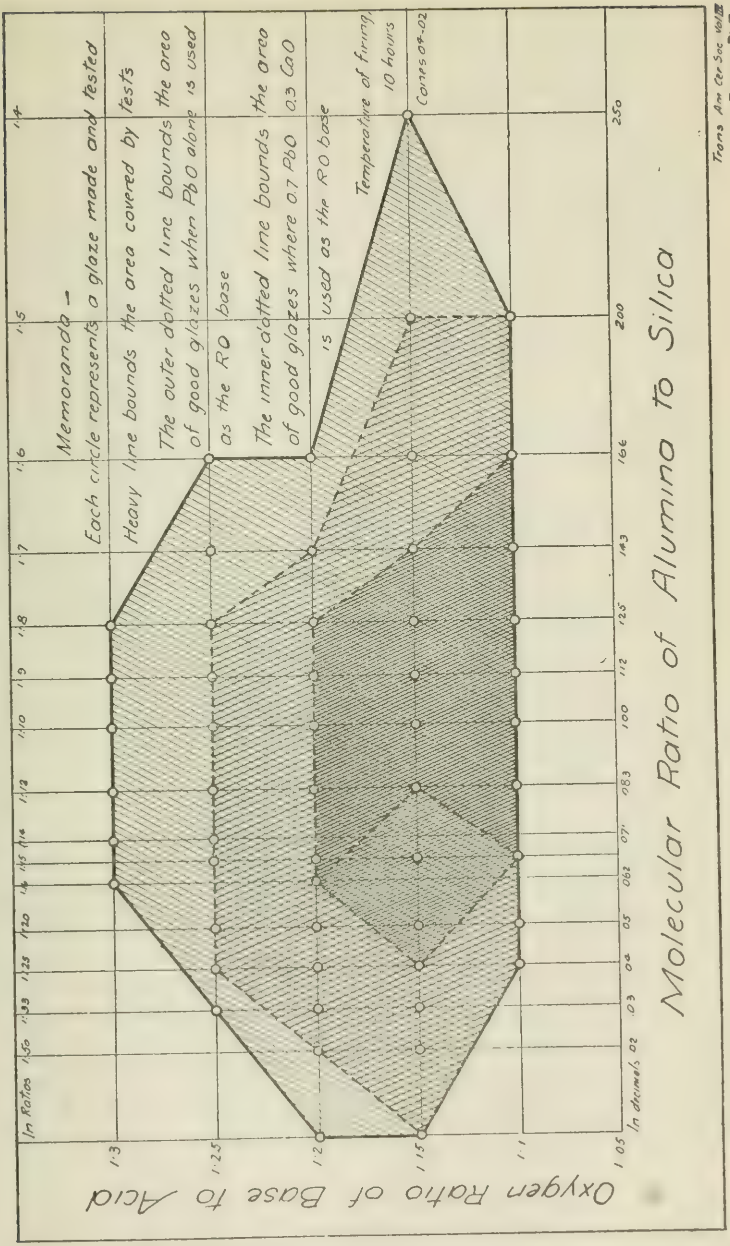
There were a number of poor and worthless glazes made and, by marking each circle with the character of its glaze on firing on each body, and with each different RO base employed, and connecting the outside members by dotted lines two areas are marked out, which represent the areas respectively of the glazes which are good, using PbO as a base, and with 0.7 PbO and 0.3 CaO as a base.

CHART SHOWING BEHAVIOR OF RAW-LEAD GLAZES ON DRY-PRESSED WHITE TILE



Trans Am Cer Soc Vol III
Fickes Pl 1

CHART SHOWING BEHAVIOR OF RAW-LEAD GLAZES ON DRY-PRESSED RED TILE



Trans Am Cer Soc Vol IX
Fickes Pl II

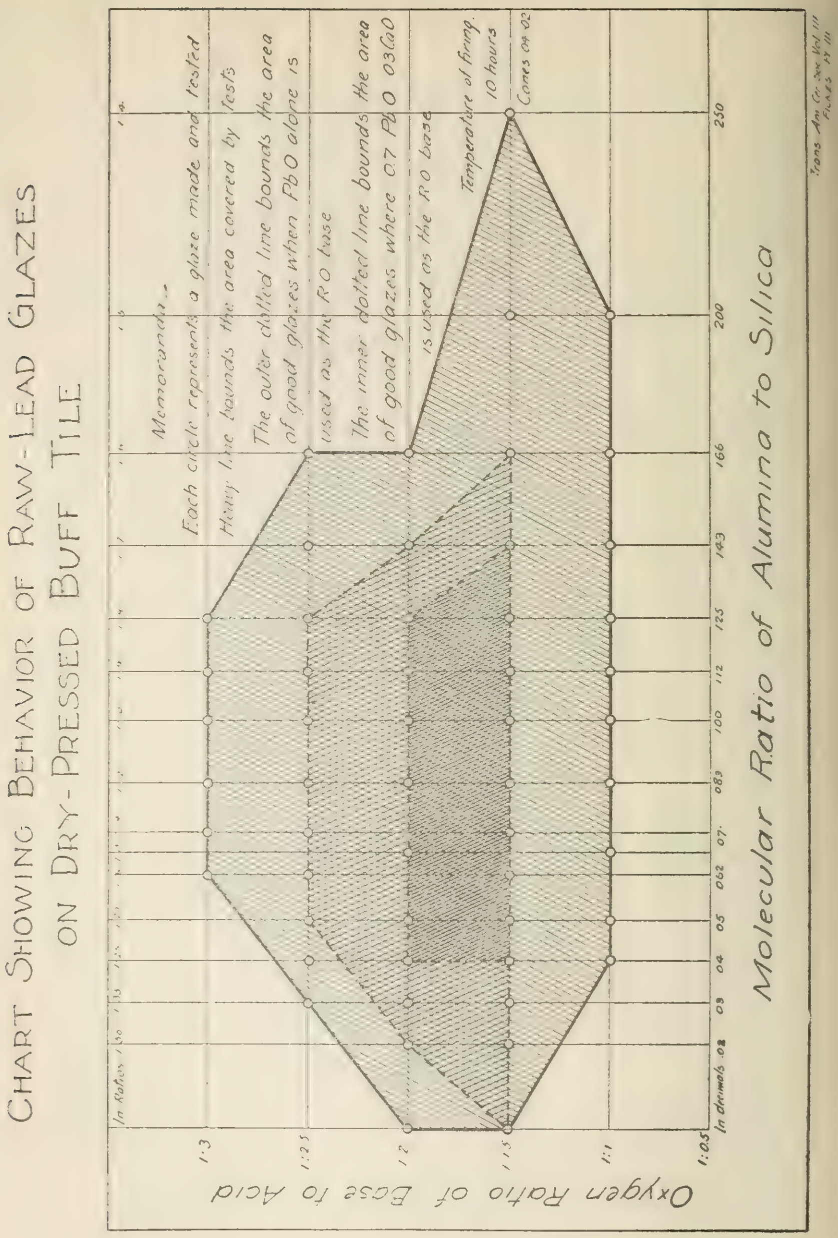


CHART SHOWING BEHAVIOR OF RAW-LEAD GLAZES
ON DRY-PRESSED BUFF TILE
Memoranda:-
Each circle represents a glaze made and tested
Heavy line bounds the area covered by tests
The outer dotted line bounds the area
of good glazes when PbO alone is
used as the RO base
The inner dotted line bounds the area
of good glazes where 0.7 PbO 0.3 CaO
is used as the RO base
Temperature of firing.
10 hours
Cones 04 02
In Ratios 1:50 1:33
In decimals .02
03
04
05
062
07
083
100
112
125
143
166
200
250
1:3
1:25
1:2
1:15
1:1
1:0.5
Oxygen Ratio of Base to Acid
Molecular Ratio of Alumina to Silica
Trans Am Cer Soc Vol III
Glazes PY III

It must be understood that a term "good glaze" in this paper refers only to those glazes which run clear, smooth and bright, and does not refer to fitting the body, ability to stand on tilted ware, attacking the body, or to any other point usually entering into the valuation of a glaze by a potter. The assumption in this paper is, that any glaze mixture that will produce a clear, glassy coating would, on a suitable body, be a perfect glaze, and would, under some possible condition, be of commercial value.

From these three plates we may readily see that:

First. The boundaries of the areas of good glazes on the three bodies are similar in shape.

Second. On the two softer bodies, red and white, the good areas are approximately equal. On the hard body, the buff, the area is much smaller.

Third. The area covered by the lead-lime and pure lead glazes producing good results, though somewhat different in size, are nearly similar in shape.

Fourth. No good glazes are produced on any body with an oxygen ratio of 1 : 3.

Fifth. Between the oxygen ratios of 1 : 1.5 and 1 : 2, we have more good glazes than with any other ratios.

Sixth. It is probable that good glazes can be produced with oxygen ratios less than 1 : 1, but this portion of the field has not been investigated.

Seventh. It is probable that the lines connecting the extreme ratios, known to be good, are not straight lines, but curve outward towards the best glazes of the next highest ratios.

Eighth. The molecular ratios from 1 : 8 to 1 : 15, inclusive, show themselves to be in general, on the three types of body employed, the most available working limits, and to be in less danger from undissolved matter or crystaline segregations, than any other ratios, either greater or less.

We may, therefore, conclude that, under the conditions of this series of experiments, the shaded areas represent the limits of composition, using the given RO elements, within which clear, brilliant, smooth-flowing glazes may be expected.

The experiments show in reference to the molecular ratio that:

First. Those glazes having a high (Al_2O_3: SiO_2) ratio, (*i. e.*, which contain only small quantities of alumina) have a tendency, especially when they occur in conjunction with the higher oxygen ratio, to form crystaline spots.

Second. Those glazes, having very low molecular ratios, and which are, therefore, high in clay substance, are unable to take into solution all the silicate matter which is present, and the result is a scummy, unfused appearance.

Series 48 and 54 were made up to find out the effect of the entire absence of alumina from the raw glaze. In one glaze of each of these two series, neither silica nor alumina were added.

Series No. 48 was almost anomalous throughout, and was contrary to all expectations. The glaze, containing only RO (0.7 PbO, 0.3 CaO), derives sufficient alumina and acid from the body to form good glazes on all trials. With 0.1 SiO_2 and no alumina the glaze becomes bad on two trials, but remains good on the red bodies. With 0.2 SiO_2, and up to 0.4 SiO_2, the glazes gradually improve on the white body to good glazes. On the red body, they gradually become spotted with crystaline matter. On the buff body, while not becoming good glazes, they do grow smoother as the SiO_2 increases. I can offer no explanation of this, other than it is due to some difference in the bodies, but what this difference is I cannot say.

In series 54, on the white body, glazes gradually improve from no glaze at all, with lead alone, to good, perfect glazes with 0.4 SiO_2. A similar improvement is noted on the buff body, but no good glazes are found.

The red body shows good, glassy surfaces, but when very little or no silica was provided by the glaze itself, the glazes are marred by dark spots where the glazes have eaten into the body. This series is much more natural than series No. 48, for it shows that the buff and white bodies are unable to supply sufficient alumina and silica to the lower members of this series to form good glazes. On the other hand, the red body is easily attacked by the glazes and gives up sufficient alumina and silica to form glasses. The buff body is so hard that not one glaze, even 0.4 SiO_2 has derived enough alumina from the body to become perfect.

CONCLUSIONS.

The work done has been very incomplete and, like most investigations, is only sufficient to cause in the investigator a lively desire to do the whole work over again in the light of the experience gained.

The tests should be pushed further, in the direction of glazes of still lower oxygen ratio than 1 : 1, for, on dry-pressed wares, either red or white, it will be observed that a great number of good glazes were obtained. The limits, at the higher acidities, both in the direction of lower and higher alumina, have been probably pretty well defined.

Also the next experimenter should especially conduct his investigation on plastic-clay trial pieces of a fair degree of vitrification. The writer is convinced that the extended area of the good glazes on white and red bodies proceeds largely from the ease with which a glaze is able to supply its wants in SiO_2 and Al_2O_3 from the dry-pressed body.

The buff body, on the other hand, is much more vitreous and close-grained, and resembles a plastic-clay body in texture. On that body, the area of good glazes is much more clearly defined in all directions; this looks as if plastic-pressed, pottery bodies, fairly hard burnt, would permit very much less latitude in the area of possible good glazes.

The remarkable reduction in the area covered by workable glazes, which followed when 0.3 CaO substitutes 0.3 PbO in the RO base is most instructive. At the same time this greatly reduced area is undoubtedly much nearer the ordinary practice of the pottery industry that the very extended field where lead alone is the fluxing base. The latter is not a common condition in the ceramic indstries.

The next experimenter, to take up this field, should try the same general plan, but with other standard and well-known combinations of RO fluxes, such as:

$$\left.\begin{array}{l}0.7\ \text{PbO}\\0.15\ \text{K}_2\text{O}\\0.15\ \text{CaO}\end{array}\right\}\text{ or }\left.\begin{array}{l}0.60\ \text{PbO}\\0.20\ \text{K}_2\text{O}\\0.10\ \text{CaO}\\0.10\ \text{ZnO}\end{array}\right\}$$

Notwithstanding the incompleteness of this work, and the many questions which it has opened up without answering, we still think it has pretty clearly established the following facts:

First. That the limits shown in plates I, II and III represent the correct limits, within which will be found all of the good glazes available, under the conditions as to temperature, bodies, burning, etc., assumed for these experiments.

Second. That good, perfect glazes can be produced with other oxygen ratios other than 1 : 2. But that, between the oxygen ratios of 1 : 1.5 and 1 : 2, we have the safest field for raw lead glazes.

Third. That the molecular ratio exercises an important function in controlling the hardness of the glaze and in controlling the tendency to crystalization during cooling.

Fourth. That, between the molecular ratios of 1 : 8 and 1 : 15, there is more probability of forming a good, perfect glaze, regardless of oxygen ratios and RO element, than with any other numbers.

Fifth. That, for each combination of RO elements, composing the fluxing base of a glaze, the limits within which good glazes can be produced, will depend on its fluxing power. The richer in lead, the wider the limits.

DISCUSSION.

Mr. Stover: As I understand, Mr. Binns, this whole series of trials are uniform with what you used last year?

The Chair: Yes, sir.

Mr. Stover: But going farther down the scale?

The Chair: But nothing to deny the results attained last year. In fact, the ratio of one to two of oxygen, which Mr. Fickes recommends, is exactly the same thing as was brought out last year as the molecular mono-silicate.

A NEW GLAZE-FLUX.

BY

WILLIAM CANNAN, Jr.

In the spring term of the school year 1899-1900, at the Ohio State University, Mr. Ira Williams, B. Sc., now instructor in Ceramics at the Iowa State University, and I elected as our experimental work, to be carried under Prof. Orton's direction, the replacement of the oxide of lead (PbO) by the available RO oxides in clear, colorless, raw-lead glazes, maturing between cones 04-02.

The more common RO oxides which form clear, colorless glazes are oxide of lead (PbO), oxide of barium (BaO), oxide of strontium (SrO), oxide of potassium (K_2O), oxide of zinc (ZnO), oxide of sodium (Na_2O), oxide of calcium (CaO), oxide of magnesium (MgO). Oxide of strontium (SrO) was not considered, because it is not on the market in quantity to make it a source of commercial supply at this time. Oxide of sodium (Na_2O) was not used because there is very little soda-feldspar on the market, and even that is not of a uniform composition. The other available salts of sodium are the carbonate of sodium, the chloride of sodium, and the nitrate of sodium, all of which are soluble, and cannot be used with any degree of accuracy without fritting.

Therefore the RO oxides used in these experiments were confined to the oxides of lead, barium, potassium, zinc, calcium and magnesium. These oxides were derived from the following sources: the oxide of lead from commercial white lead, the oxide of barium from commercial barium carbonate, the oxide of potassium from potash feldspar, the oxide of zinc from commercial oxide of zinc, the oxide of calcium from English whiting (calcium carbonate), the oxide of magnesium from commercial magnesium oxide.

THE DESIGNING OF THE SERIES OF GLAZES.

In the construction of the series, we kept in mind Dr. Seger's statements on the fusibility of the RO oxides with which we were to work, which is as follows in the order of their fusibility: oxide of lead, the most fusible, oxide of barium, oxide of potassium, oxide of zinc, oxide of calcium and oxide of magnesium, the most infusible. The fusibility of the RO oxides, according to Dr. Seger, you will notice is as their molecular weight, the heaviest being the most fusible, and the lightest the most infusible. We replaced so much oxide of lead with each RO oxide in turn that we were sure that it would not mature under the conditions assumed for the test, and used this hard glaze as one extreme of each series, and a pure lead glaze as the soft extreme of the same series.

These we followed with series in which two oxides were used at once in replacing lead and then three, four and finally five oxides at once.

The details are explained more closely as follows: *First.* We took as the type glaze, one in which PbO is the only fluxing oxide, having the formula PbO 0.2 Al_2O_3, 2.0 SiO_2, which we called No. 300.

Second. We constructed a series for each of the oxides used, in each of which the glaze No. 300, PbO, 0.2 Al_2O_3, 2.0 SiO_2 was the soft end, and the hard end was made according to the oxide used.

The following is an example of one of the single replacement series.

SINGLE REPLACEMENT SERIES.

Glaze No.	Molecular Formulae of Glazes.			
	K_2O	PbO	Al_2O_3	SiO_2
300	....	1.00	0.20	2.00
301	0.05	0.95	0.20	2.00
302	0.10	0.90	0.20	2.00
303	0.15	0.85	0.20	2.00
304	0.20	0.80	0.20	2.00

In glaze No. 304, we introduced all the Al_2O_3 from spar; this was done to get as much K_2O in the glaze as possible.

In each of the single replacement series, the equivalent of the replacing oxide increased by 0.05 equivalent and the PbO decreased by 0.05 equivalent up to the hardest glaze for each different oxide; these extremes, or "hard ends" had the following formulæ:

Lime Lead Series...	0.6 CaO	0.4 PbO	0.20 Al_2O_3	2.00 SiO_2
Magnesia Lead Ser.	0.4 MgO	0.6 PbO	0.20 Al_2O_3	2.00 SiO_2
Barium Lead Series	0.5 BaO	0.5 PbO	0.20 Al_2O_3	2.00 SiO_2
Zinc Lead Series...	0,3 ZnO	0.7 PbO	0.20 Al_2O_3	2.00 SiO_2

Third. We constructed ten "double replacements," or series in which two oxides were used to replace the PbO. In all of these double replacement series, K_2O was used, and was a constant quantity throughout each series. The following is an example of one of these double replacement series.

DOUBLE REPLACEMENT SERIES.

Glaze No.	Molecular Formula of Glazes.				
	K_2O	CaO	PbO	Al_2O_3	SiO_2
301	0.05	0.00	0.95	0.20	2.00
336	0.05	0.05	0.90	0.20	2.00
337	0.05	0.10	0.85	0.20	2.00
338	0.05	0.15	0.80	0.20	2.00
339	0.05	0.20	0,75	0.20	2.00
340	0.05	0.25	0.70	0.20	2.00
341	0.05	0.30	0.65	0.20	2.00
342	0.05	0.35	0.60	0.20	2.00

Three other series similar to this were made, in which K_2O and CaO replaced the PbO, and in each advancing series, the K_2O increased by 0.05 equivalent, but remained constant throughout the series. In these series, the CaO increased by 0.05 equivalent and the PbO decreased by 0.05 equivalent, with each successive glaze. This gave us four series of potash-lime replacements, in which the lead and lime fluctuated in each glaze, and the other elements remained fixed.

0.05 K_2O	CaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2
0.10 K_2O	CaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2
0.15 K_2O	CaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2
0.20 K_2O	CaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2

Two series similar to these were constructed for each MgO, BaO, ZnO. The following is an example of each series:

MgO double replacement series:

Low Potash, 0.10 K_2O, MgO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2
High Potash, 0.20 K_2O, MgO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2

BaO double replacement series:

Low Potash, 0.10 K_2O, BaO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2
High Potash, 0.20 K_2O, BaO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2

ZnO double replacement series:

Low Potash, 0.10 K_2O, ZnO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2
High Potash, 0.20 K_2O, ZnO and PbO fluctuating 0.20 Al_2O_3 2.00 SiO_2

Fourth. We constructed "triple replacement" series, or series in which three oxides were used simultaneously to replace PbO. In all of the triple replacement series, K_2O and CaO were used, and each was constant through each series. The following is an illustration:

TRIPLE REPLACEMENT SERIES.

Glaze No.	Molecular Formula of Glazes.					
	K_2O	CaO	MgO	PbO	Al_2O_3	SiO_2
344	0.10	0.10	0.00	0.80	0.20	2.00
386	0.10	0.10	0.05	0.75	0.20	2.00
387	0.10	0.10	0.10	0.70	0.20	2.00
388	0.10	0.10	0.15	0.65	0.20	2.00
389	0.10	0.10	0.20	0.60	0.20	2.00
390	0.10	0.10	0.25	0.55	0.20	2.00
391	0.10	0.10	0.30	0.50	0.20	2.00

Series similar to this were made in which BaO and ZnO were each in turn combined with fixed quantities of K_2O and CaO, in replacing lead oxide, of which the following schedule gives the key:

0.15 K_2O	0.15 CaO	BaO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2
0.20 K_2O	0.20 CaO	ZnO and PbO Fluctuating.	0.20 Al_2O_3	2.00 SiO_2

Fifth. We constructed three "quadruple replacement" series, or series in which four oxides were used at once to replace PbO, in which the K_2O and CaO were constant throughout each series.

QUADRUPLE REPLACEMENT SERIES.

Glaze Number	Molecular Formula of Glazes.						
	K_2O	CaO	MgO	ZnO	PbO	Al_2O_3	SiO_2
530	0.10	0.10	0.050	0.050	0.70	0.20	2.00
531	0.10	0.10	0.075	0.075	0.65	0.20	2.00
532	0.10	0.10	0.100	0.100	0.60	0.20	2.00
533	0.10	0.10	0.125	0.125	0.55	0.20	2.00
534	0.10	0.10	0.150	0.150	0.50	0.20	2.00
535	0.10	0.10	0.175	0.175	0.45	0.20	2.00
536	0.10	0.10	0.200	0.200	0.40	0.20	2.00
537	0.15	0.15	0.050	0.050	0.60	0.20	2.00
538	0.15	0.15	0.075	0.075	0.55	0.20	2.00
539	0.15	0.15	0.100	0.100	0.50	0.20	2.00
540	0.15	0.15	0.125	0.125	0.45	0.20	2.00
541	0.15	0.15	0.150	0.150	0.40	0.20	2.00
542	0.15	0.15	0.175	0.175	0.35	0.20	2.00
543	0.15	0.15	0.200	0.200	0.30	0.20	2.00
544	0.20	0.20	0.050	0.050	0.50	0.20	2.00
545	0.20	0.20	0.075	0.075	0.45	0.20	2.00
546	0.20	0.20	0.100	0.100	0.40	0.20	2.00
547	0.20	0.20	0.125	0.125	0.35	0.20	2.00
548	0.20	0.20	0.150	0.150	0.30	0.20	2.00

In these three series, the fluctuating replacing oxides, MgO and ZnO, each advanced through the series by .025 equivalent, the sum of which equals 0.05 equivalent of PbO replaced.

Sixth. We constructed a "quintuple replacement" series, or one in which the PbO was replaced by all the oxides we were experimenting with, simultaneously. In this series, K_2O CaO were constant throughout the series, the other oxides, MgO, ZnO, BaO each increasing by 0.025 equivalent.

QUINTUPLE REPLACEMENT SERIES.

Glaze No.	Molecular Formulae of Glazes.							
	K_2O	CaO	MgO	ZnO	BaO	PbO	Al_2O_3	SiO_2
549	0.20	0.20	0.025	0.025	0.025	0.525	0.20	2.00
550	0.20	0.20	0.050	0 050	0.050	0.450	0.20	2.00
551	0 20	0.20	0.075	0.075	0.075	0.375	0.20	2.00
552	0.20	0.20	0.100	0.100	0.100	0.300	0.20	2.00
553	0.20	0.20	0.125	0.125	0.125	0.225	0.20	2.00

In this series the equivalent PbO decreased by 0.075 equivalent throughout the series, 0.075 being the sum of the fluctuating replacing oxides.

THE MANUFACTURE OF THESE GLAZES.

First. We made up five hundred grams of each glaze throughout these experiments.

Second. We ground the extremes of each series, and blended the intermediates from these extremes. Each extreme was ground for one hour in a ballmill, and as all intermediate glazes were made by mixing the extremes, all glazes were equally ground.

The amount of each extreme which was needed was found in the following manner. For example of this we will refer back to series No. 25 which has been already given in the single replacement table.

We want 500 grams of each of these glazes; therefore, the combined weight of the series will be 500 grams × 5 = 2500 grams + 500 grams for loss in screening and blending = 3000 grams of stock required. 3000 grams ÷ 2 = 1500 grams, which is the number of grams of Nos. 300 and 304 glazes which must be ground to produce this series

The calculation of the batch weights of the extremes of each series was done as follows, using glaze No. 304 as an example:

GLAZE 304.

Glaze	$0.20\ K_2O$	$0.8\ PbO$	$0.20\ Al_2O_3$	$2.00\ SiO_2$
0.2 Eqv. Spar	$0.20\ K_2O$		$0.20\ Al_2O_3$	$1.20\ SiO_2$
Difference	×	$0.8\ PbO$	×	$0.8\ SiO_2$
0.8 Eqv. White Lead	×	$0.8\ PbO$	×	
Difference	×	×	×	$0.8\ SiO_2$
0.8 Eqv. Flint	×	×	×	$0.8\ SiO_2$

Calculating the parts of each ingredient to be used:

0.20 Eqv. Spar × 556 = 111.2 × 4.102 = 456.14 grams Spar.
0.80 Eqv. W. Lead × 258 = 206.4 × 4.102 = 846.65 grams White Lead.
0.80 Eqv. Flint × 60 = 48.0 × 4.102 = 196.90 grams Flint.
Total parts in Glaze = 365.6 1499.69 grams No. 304 Glaze

We want 1500 drams of this glaze, therefore 1500 g ÷ 365.6 equals 4.102, the factor by which we must multiply the parts of each ingredient in the glaze to get the batch weights for 1500 drams.

The extremes were weighed out, and ground the required time, in small porcelain ball mills, after which the material was screened through a 150 mesh screen, the slip was then let stand until the solid matter settled, after which the supernatant water was syphoned off. After the water had been syphoned off, and the mass thus concentrated, we mixed each thoroughly and made a moisture test to determine the percentage of solid matter in each slip. For example of determining the percent of solid matter in a slip, and how we used the same in blending each series of glazes, we will take the extremes of series 25, glazes 300-304, from which were blended glazes 301, 302 and 303.

To determine the solid matter in glazes 300 and 304, we mixed each slip thoroughly, and took a sample of each in a dish of known weight.

	Glaze 300	Glaze 304
Weight of dish and slip............	199.0 grams	195.0 grams
Weight of dish	78.2 grams	74.9 grams
Weight of slip	120.8 grams	120.1 grams

We then dried on a steam bath till perfectly dry and re-weighed.

	Glaze 300	Glaze 304
Weight of dish and dry glaze	116.1 grams	115.7 grams
Weight of dish	78.2 grams	74.9 grams
Weight of dry glaze	37.9 grams	40.8 grams
Percent of solids in slips...	$\frac{37.9 \text{ x } 100}{120.8}=31.37\ \%$	$\frac{40.8 \text{ x } 100}{120.1}=33.97\ \%$

The factor by which a given amount of a dry body must be multiplied to get the slip weight of same, is found by dividing one by the percentage of solid matter in the slip, or by dividing the slip weight of a moisture test by the dry weight of the same. The slip of glaze No. 300 is 31.37 per cent. solid matter. By dividing one by 31.37, the per cent. of solid matter in slip, we have $\frac{1.0}{.3137}$ = 3.187, or by the second method, the weight of the slip in moisture test 120.8 : 37.9, the dry weight in the moisture test, equals $\frac{120.8}{37.9}$ equals 3.187.

The factor by which a given amount of glaze No. 304 must be multiplied to find the slip weight, produced by the second method of calculation equals $\frac{120.1}{40.8}$ = 2.94.

BLENDING TABLE.

No. Glaze	Proportions		Weight ot Slip to be Used	
	Glaze 300	Glaze 304	Glaze 300	Glaze 304
300	All	None	500×1.00×3.187 = 1593.5	500×0.00×2.94 = 000.0
301	3	1	500×0.75×3.187 = 1195.1	500×0.25×2.94 = 366.5
302	2	2	500×0.50×3.187 = 796.7	500×0.50×2.94 = 735.0
303	1	3	500×0.25×3.187 = 398.3	500×0.75×2.94 = 1102.5
304	None	All	500×0.00×3.187 = 000.0	500×1.00×2.94 = 1370.0

The glazes were then blended according to the slip weights thus obtained, after which each glaze was placed in an ordinary self-sealing fruit jar, well shaken, sieved and replaced in the jar till used.

THE DIPPING.

The glazes after blending, were left standing for a while, generally until the day before we intended to fire, at which time we dipped the trial pieces. Before dipping, we poured off the supernatant water, shook the glaze well in the receptacle, poured through a 100-mesh screen into the dipping receptacle. This screening of the glaze was done to remove any foreign matter that might have got into the glaze, and to insure that the glaze was thoroughly mixed. After screening, the glaze was brought to a proper consistency by the addition of water. The glaze was dipped about one-sixteenth of an inch thick on each trial piece, which was previously marked in blue stain with the number of the glaze in which it was to be dipped.

The trial pieces used were biscuit floor-tile ½ x 1 x 4 inches, which were of three degrees of hardness and of three colors, a white body which was quite soft, a red body which was of medium hardness and a buff body which was much the hardest being nearly vitrified. These floor tile were furnished by the United States Encaustic Tile Company, of Indianapolis, Indiana.

After drying, the trials were put in well luted setters, each body and series being put in a setter by itself, except in the series where the number of glazes was more than a setter would hold, in which case a series would be put in two setters, but always keeping the bodies separate. For example, we will take series No. 25, which has five glazes. Into each of these five glazes were dipped three trials of each body, white, red and buff. These were put into setters as follows: Three setters of each white, red and buff body; one setter of each body for the three divisions of the kiln. The setters were now put in such tiers and placed in the kiln in such a position as would place the bodies and glaze series as before described, after which the wicket was closed and the firing begun at once.

THE FIRING OF THE TRIALS.

The trials were burnt under tile-setters similar to those used by the U. S. Encaustic Tile Co., the gaps between setters being well luted. The total length of the burn was approximately 22 hours, which was divided into two periods. The first period was devoted to raising the temperature to cone 04, which covered about twelve hours. The second period was devoted to heat soaking or maturing of the glaze, for which about ten hours was used, during which time the temperature was kept between cones 04-02. To accomplish these conditions as nearly as possible, we used a Chatelier Pyrometer and fired the kiln according to a time temperature curve adopted in advance.

The kiln used in these experiments was a down draft kiln, the dimensions of whose ware-chamber were, height 5 feet 9 inches, length 2 feet 7 inches, width 22 inches. The kiln was divided into three parts or zones, upper, middle and lower. The parts were separated more or less by placing fire brick on edge between the setters of the lower and middle parts, and the middle and upper parts. The fuel used was gas coke and coal, just as little coal as possible being used.

The firing was done as follows: The fire was started with wood and coal, just enongh of the latter being used, to get a good grate of live fire, after which the gas coke was used, until the point was reached where it would not burn fast enough under full draft to give the desired rise in temperature. At this point soft coal was put on the coking plate of the furnace. The volatile matter in the coal was gradually roasted out, and mingled with and consumed part of the oxygen of the excess air coming in through the bed of hot coke. After the volatile matter was all gone from the coal, or in other words after the coal was coked, it was shoved back on the grates, and a new charge of coal was put on the coking plate. The coke thus produced was not sufficient to keep the grates covered, so we used as much gas coke as was needed for this purpose, and only used the coal as fast as was needed to raise the heat.

An ideal time-temperature curve was first plotted as follows: We took a co-ordinate paper and plotted on the horizontal line, one hour to each square; and on the vertical line we let each square equal 100 deg. C. Then we counted off 12 hours on the time line, and up to the temperature of cone 04 on the temperature line which is approximately 1070 deg. C.; where these two lines intersected we made a mark and from zero to this mark we drew a line which represented graphically the progress our temperature was supposed to make for the first 12 hours. From this same point, we also drew a line to the intersection of 22 hours, and the temperature of cone 02, approximately 1110 deg. C., which represented the course our temperature should follow during the heat soaking period.

This curve was used as follows: When the fire was started in the kiln, the time was noted, and this took the place of zero on the time side of the curve; from this we marked on each square the time we would arrive at it. While we were raising the heat and until we got near the melting point of cone 04, we took the temperature reading every half hour, after which we took the temperature reading every fifteen minutes until the end of the burn, and plotted same on the curve sheet, aiming, as you well understand, to have these readings come as near the ideal curve as possible.

If you have never tried to fire a small kiln by one of these curves, you cannot appreciate how difficult it is to follow the ideal curve. The curve that is plotted from the readings will be more or less of a jagged line, the best that can be done, and especially so, when it comes to the heat-soaking, or holding of the temperature constant between cone 04 and 02.

After the burn was finished, the fire was pulled and put out with water. This was done to eliminate as much as possible the sulphur fumes that escape into the kiln, if the fire is allowed to die out on the grates.

The cooling of the kiln was done as rapidly as possible with an open damper.

After cooling, the kiln was emptied, the setters unpacked and each trial piece marked either T, M or B, meaning top, middle and bottom, the three sections of the kiln.

The trials were then taken to a large table, on which they were placed in position in series, each kind of body by itself and in the same relative position they occupied in the kiln. This arrangement gave us very good opportunity to study the different points of each series of glazes, and also compare the different series.

THE STUDY OF THE GLAZES.

The glazes were studied from four points of view, only one of which I will discuss here.

First. The fusion of the series.

Second. The highest workable replacement of PbO in each series.

Third. The appearance of the glazes, as to color and brightness.

Fourth. As to crazing and shivering.

According to Dr. Seger's notes, we had a right to expect a higher replacement of the PbO by the heavier oxides than the lighter oxides, but this was not the case. For example, BaO, which is next to PbO in weight, was the least promising of all the oxides. In all the combinations in which we used it, the glazes were scummy and worthless; a very good example of this occurs in the quintuple replacement series in the second glaze in the series, which has the formula:

$0.2\ K_2O$, $0.2\ CaO$, $0.05\ MgO$, $0.05\ ZnO$, $0.05\ BaO$, $0.45\ PbO$, $0.2\ Al_2O_3$ $2.00\ SiO_2$.

This is not a workable glaze, while we have a glaze of the same composition, excepting the BaO, which is a perfect and workable glaze, as far the combination of oxides is concerned.

No single glaze which BaO was used in any proportion was workable, and they are left out of further consideration on that account.

The following are the formulæ of the highest workable replacements of PbO, in the different combinations we have tried:

HIGHEST SUCCESSFUL SINGLE REPLACEMENTS.

$0.15\ K_2O$	$0.85\ PbO$	$0.20\ Al_2O_3$	$2\ 00\ SiO_2$	All bodies.
$0.15\ ZnO$	$0.85\ PbO$	" "	" "	Red and white bodies.
$0.20\ ZnO$	$0.80\ PbO$	" "	" "	White bodies.
$0.15\ MgO$	$0.85\ PbO$	" "	" "	White bodies.
$0.30\ MgO$	$0\ 70\ PbO$	" "	" "	Red and buff bodies.
$0.25\ CaO$	$0.75\ PbO$	" "	" "	White bodies.
$0.20\ CaO$	$0.80\ PbO$	" "	" "	Red and buff bodies.

HIGHEST SUCCESSFUL DOUBLE REPLACEMENTS USING POTASH AND LIME.

$0.05\ K_2O$	$0.15\ CaO$	$0.8\ PbO$	$0.2\ Al_2O_3$	$2.00\ SiO_2$
$0.10\ K_2O$	$0.20\ CaO$	$0.70\ PbO$	" "	" "
$0.15\ K_2O$	$0.20\ CaO$	$0.65\ PbO$	" "	" "
$0.20\ K_2O$	$0.20\ CaO$	$0.60\ PbO$	" "	" "

HIGHEST SUCCESSFUL TRIPLE REPLACEMENTS.

$0.1\ K_2O$	$0.1\ CaO$	$0.15\ MgO$	$0.65\ PbO$	$0.2\ Al_2O_3$	$2.00\ SiO_2$
$0.2\ K_2O$	$0.2\ CaO$	$0.20\ ZnO$	$0.40\ PbO$	" "	" "

HIGHEST SUCCESSFUL QUADRUPLE REPLACEMENTS.

0.1 K_2O	0.1 CaO	0.15 MgO	0.15 ZnO	0.50 PbO	0.2 Al_2O_3	2.00 SiO_2
0.15 K_2O	0.15 CaO	0.15 MgO	0.15 ZnO	0.40 PbO	" "	" "
0.2 K_2O	0.20 CaO	0.15 MgO	0.15 ZnO	0.30 PbO	" "	" "

A study of these results will show that the workable limit of replacement of PbO by the different oxides and combinations of oxides used, with the exception of oxide of barium, lies between 0.15 and 0.2 equivalent of each oxide. Another thing developed from the study of these glazes is, that the more oxides used together in replacing PbO, the less lead is required, and the clearer, brighter and smoother the glaze becomes. This is especially noticeable in its effect on the color of the red body. The body does not seem to get darker in color under a quadruple or triple replacement glaze as it does under a single or double replacement glaze.

It will be observed that in these tests, that the oxide of magnesium showed a fluxing power quite different from that which Seger's law led us to expect. We are there told that MgO is the most refractory of the fluxes and barium the least so, next to lead. Whatever the cause may be, we find that BaO is not of any value in these raw glazes in any proportion, while MgO, which we expected to find well nigh useless, gives a higher single replacement than any other oxide and works well in every combination in which it is placed

THE NEW GLAZE FLUX.

One year later, in discussing the subject of this work, the following idea was suggested by Professor Orton. In Ohio we have an abundance of dolomitic limestones, that are nearly pure dolomites. These stones are burned, slaked, dried and bolted and the product used as a whitening for mortar, and also used in the manufacture of prepared plaster. If this material is constant in composition, and does not contain iron enough to color a glaze, why not use it as a glaze flux in the place of English whiting?

The above tests show that MgO is a valuable flux in itself and will replace lead as well as CaO, and also, they show that the more fluxes are present at once, the lower the lead may be brought without losing quality in the resultant glaze. If this is the case, we ought to get an additional advantage from the use of a dolomite over a pure lime like the English chalk, in the fact of its higher substituting power in a lead glaze. Also there is a heavy advantage in price, as the powdered Ohio lime can be had for $7.50 per ton against about $20.00 for English whiting.

I took this suggestion up, and have made the following investigation of the possibilities in this direction.

On request, the following firms sent samples of their products: The Seneca White Lime Company, Fostoria, Ohio. The Buckeye Lime Company of Toledo, Ohio.

The analysis of the material used was as follows:

SiO_2	.030%
Fe_2O_3	Trace
Al_2O_3	Trace
$Ca(OH)_2$	56.59%
$Mg(OH)_2$	41.59%
	98.28%

I am indebted to Mr. S. V. Peppel, B. Sc., Laboratory Assistant in the Ceramic Department for 1900-01, for the analysis. The deficit in this analysis is thought to be due to CO_2 which the hydroxides readily take up from the air, and it was not thought to be necessary to determine the same at this time.

Transforming the percentage composition of this analysis into the molecular formula, we have:

$$\left.\begin{array}{l} 0.51\ Ca\ (OH)_2 \\ 0.49\ Mg\ (OH)_2 \\ \hline 1.00\ RO \end{array}\right\}\ 0.003\ SiO_2$$

The SiO_2 was not considered in calculating the batch weights, its proportion being so small.

To investigate the advisability of using this new material in glaze combinations, in place of English whiting, I decided to make nine series of glazes, parallel to the series described heretofore, using this combined hydrate of calcium and magnesium in each, equivalent for equivalent, in place of English whiting.

The following is an example of how this new material was used in place of English whiting.

DOUBLE REPLACEMENT, USING ENGLISH WHITING.

K_2O	CaO	PbO	Al_2O_3	SiO_2
0.15	0.00	0.85	0.20	2.00
0.15	0.05	0.80	"	"
0.15	0.10	0.75	"	"
0.15	0.15	0.70	"	"
0.15	0.20	0.65	"	"
0.15	0.25	0.60	"	"
0.15	0.30	0.55	"	"

DOUBLE REPLACEMENT, USING OHIO DOLOMITE.

K_2O	Dolomite.	PbO	Al_2O_3	SiO_2	Equivalents of CaO and MgO Introduced in the Dolomite	
					CaO	MgO
0.15	0.00	0.85	0.20	2.00	0.00	0.000
0.15	0.05	0.80	"	"	0.026	0.024
0.15	0.10	0.75	"	"	0.051	0.049
0.15	0.15	0.70	"	"	0.077	0.073
0.15	0.20	0.65	"	"	0.103	0.097
0.15	0.25	0.60	"	"	0.129	0.121
0.15	0.30	0.55	"	"	0.154	0.146

The following are the extremes of each series in which dolomite was used :

SINGLE REPLACEMENT SERIES.

Dolomite	PbO	Al_2O_3	SiO_2	Dolomite Equals	
				CaO	MgO
0.00	1.00	0.20	2.00	0.000	0.000
0.60	0.40	"	"	0.306	0.294

DOUBLE REPLACEMENT SERIES.

K_2O	Dolomite	PbO	Al_2O_3	SiO_3	Dolomite Equals	
					CaO	MgO
0.10	0.00	0.90	0.20	2.00	0.000	0.000
0.10	0.35	0.55	"	"	0.180	0.170
0.15	0.00	0.85	"	"	0.000	0.000
0.15	0.30	0.55	"	"	0.154	0.146
0.20	0.00	0.80	"	"	0.000	0.000
0.20	0.30	0.50	"	"	0.154	0.146

TRIPLE REPLACEMENT SERIES.

K_2O	Dolomite	MgO	PbO	Al_2O_3	SiO_2	Dolomite Equals	
						CaO	MgO
0.10	0.10	0.00	0.80	0.20	2.00	0.051	0.049
0.10	0.10	0.30	0.50	0.20	2.00	0.051	0.049

K_2O	Dolomite	ZnO	PbO	Al_2O_3	SiO_2	CaO	MgO
0.20	0.20	0.00	0.60	0.20	2.00	0.103	0.097
0.20	0.20	0.20	0.40	0.20	2.00	0.103	0.097

QUADRUPLE REPLACEMENT SERIES.

K_2O	Dolomite	MgO	ZnO	PbO	Al_2O_3	SiO_2	Dolomite Equals	
							CaO	MgO
0.10	0.10	0.00	0.00	0.80	0.20	2.00	0.051	0.049
0.10	0.10	0.20	0.20	0.40	0.20	2.00	0.051	0.049
0.15	0.15	0.05	0.05	0.60	0.20	2.00	0.077	0.073
0.15	0.15	0.20	0.20	0.30	0.20	2.00	0.077	0.073

The glazes were manufactured the same as the glazes in which English whiting was used, except the burning, which was prolonged ten hours longer than the burn of the glazes in which English whiting was used. However cone 02 in the bottom of the kiln was only started, so that while the time was longer, the temperature was not in excess. Convention time was too near to make another burn possible. The glazes in which the dolomite was used, should be burnt again, and to make the comparison a better one, the glazes in which English whiting was used should be burnt at the same time.

THE COMPARISON OF THESE TWO SETS OF GLAZES.

SINGLE REPLACEMENT SERIES.

First. On the White body, the dolomite glazes show the best fusion. On the red and buff bodies, the lime glazes show the best fusion.

Second. 0.15 equivalents PbO replaced is the limit for both, on all bodies.

Third. Both give clear bright glazes.

Fourth. No crazing or shivering in either.

DOUBLE REPLACEMENT SERIES.

There are two series of each of these that are comparable, and a third series which was made, using dolomite, which is not comparable with the equivalent series, in which English whiting was used. The hard extreme of this dolomite series was what it should be, but the soft extreme, which should have developed most easily was just barely vitrified. This must have been due to some error in weighing.

In the two series, whose composition is as follows:

0.10 K_2O, Dolomite and PbO fluctuating,
and 0.10 K_2O, Whiting and PbO fluctuating,

the following facts were observed:

First. The fusion is better to the upper limit in the whiting series; the fusion of the dolomite glazes takes a sudden drop just after passing the working limit and seems to fall away faster in fluxing power.

Second. On the white body, 0.25 equivalents PbO were replaced in both series. On the red and buff bodies 0.3 equivalents PbO were replaced in both series. On all bodies, the replacement of the PbO is in favor of the series containing dolomite.

Third. Both give clear bright glazes.

Fourth. The buff body of the English whiting series is badly crazed.

In the two series, the composition of which is as follows:

0.20 K_2O, Dolomite and PbO fluctuating,
and 0.20 K_2O, Whiting and PbO fluctuating,

the following facts were observed:

First. The English whiting series shows the best fusion; being well fused to the upper limit, while the last glaze in the dolomite series is not very well fused.

Second. 0.45 equivalents of PbO is replaced on all bodies in the English whiting series. In the dolomite series, 0.40 PbO is successfully replaced.

Third. Both series give clear, bright glazes.

Fourth. The buff body English whiting series is badly crazed, while the red and white body of the same series, and buff body of the dolomite series are only slightly crazed.

TRIPLE REPLACEMENT SERIES.

There are two of these, one for MgO and one for ZnO. Beginning with the K_2O, CaO and MgO series, whose composition was 0.1 K_2O, 0.1 dolomite, MgO and PbO fluctuating, and 0.1 K_2O, 0.1 CaO from English whiting, MgO and PbO fluctuating, the following facts were observed:

First. The English whiting series is well fused to the upper limit. The dolomite series is well fused to the middle of the series on the white body, the upper numbers being drawn up into glassy beads. On the buff and red bodies, all except the last numbers are well fused.

Second. The English whiting series replaces 0.35 PbO, white and red bodies, and 0.4 PbO on buff body. The dolomite series replaces 0.35 equivalent PbO on all the bodies.

Third. Both series give clear bright glazes.

Fourth. English whiting series, two glazes crazed on the buff body.

Taking next the K_2O, CaO, ZnO series, whose composition was 0.2 K_2O, 0.2 dolomite, ZnO, PbO fluctuating and 0.2 K_2O 0.2 CaO from English whiting, ZnO, PbO fluctuating, we observed:

First. The English whiting series is perfectly fused to the upper limit. The Dolomite series well fused to the upper limit, but dry spots show on the white body, and slight undissolved matter on red and buff bodies at the upper end.

Second. The English whiting series is workable to the upper limit, in which 0.6 PbO is replaced. The dolomite series replaces 0.55 equivalents PbO, and with very little rise in the temperature, would replace 0.6 PbO.

Third. Both give clear bright glazes, with the English whiting series a little better on the white body, and the dolomitic series much the better on the red body.

Fourth. The buff body in both series is crazed, but the English whiting series is the worst.

QUADRUPLE REPLACEMENT SERIES.

There are two of these series for both English whiting and dolomitic stone. The first comparison falls with series whose composition was 0.1 K_2O, 0.1 Dolomite, MgO-ZnO, and PbO fluctuating, and 0.1 K_2O, 0.1 CaO from English whiting, MgO, ZnO and PbO fluctuating. The following results were observed:

First. The fusion is the best in the English whiting series.

Second. The English whiting series replaces 0.5 PbO in all bodies. The dolomite series replaces 0.45 PbO on red and buff bodies and 0.40 PbO in white body.

Third. Both give clear bright glazes, but this is slightly in favor of the dolomite series on the buff body, and decidedly so on the red body.

Fourth. Two members of each crazed on the buff body.

In the next two series, the K_2O was increased to 0.15, and the series became

> 0.15 K_2O, 0.15 Dolomite, MgO, ZnO, and PbO, fluctuating,
> 0.15 K_2O, 0.15 CaO from the English whiting, MgO, ZnO, and PbO fluctuating.

First. The English whiting series is well fused to the upper limit. The three upper members of the dolomite series are fused and smooth but are milky from undissolved matter.

Second. The English whiting series replaces on the white body 0.55 PbO, and 0.60 PbO on the red and buff bodies. The dolomite series replaces on the white body 0.45 PbO and 0.55 on the red and buff bodies.

Third. Both give good, clear, bright glazes, but the dolomite series has decidedly the advantage in color on the red body.

Fourth. No crazing.

To see if the trace of iron in the dolomite would affect the color of the glazes, I made three PbO-CaO glazes, introducing same from chemically pure reagents; the PbO being taken from PbO_2, the CaO from $CaCO_3$. The glazes had the formulæ:

	0.1 PbO	0.2 Al_2O_3	2.00 SiO_2
0.1 CaO	0.9 “	“	“
0.2 “	0.8 “	“	“

By comparing with this series, the corresponding glazes made from dolomite, they were found to be colorless; that is, the trace of iron in the dolomite is too minute to color a glaze in which the stone is used.

This series was burnt at the same time that the dolomite glazes were, and can be fairly compared with them as to the workable replacement of PbO.

I have said that we thought we could use a higher equivalent of this new material than of whiting, but from the comparisons of all of the foregoing series of glazes, it was found that in all except two instances, the dolomite replacement was lower than the whiting. I also stated that cone 02 in the bottom of the kiln was not brought down when the dolomite glazes were burnt, but cone 02 was brought down when the whiting series was burnt; for this reason, the comparison of the glazes is not a fair one, and the dolomite is placed at a disadvantage.

However, a comparison of the series in single replacement of dolomite and PbO was made, and this series made from C. P. reagents is a just one, as they were burnt together. The results show what was expected of the dolomite, and what the other tests failed to disclose.

Comparing these two last series, *i. e.*, the C. P. PbO-CaO series, and the dolomite-PbO series, we find:

First. The dolomite series shows good fusion up to 0.25 equivalent dolomite, while 0.1 CaO from C. P. $CaCO_3$, is not fully fused.

Second. The workable replacement of PbO by dolomite is 0.15 PbO, while 0.1 PbO replaced by CaO from C. P. $CaCO_3$ is not workable.

Third. The color and brightness is about equal.

CONCLUSIONS.

From the nine foregoing series of glazes, comprising a very large number of individual trials, I find the following:

First. That dolomite has been successfully used in place of whiting in all the various glazes here tested.

Second. That in many of the glazes made, the dolomite glaze was a better glaze in color, beauty and lack of crazing than the corresponding whiting glaze.

Third. That an unfortunate accident in the kiln management prevented a strict comparison of the two series, and that the disadvantage was against the dolomite glazes.

Fourth. That in spite of this, they are a very close second to the whiting glaze throughout, in point of fusibility and replacing power, and in occasional instances surpass them.

Fifth. That in the only absolutely fair comparison reached, the dolomite glazes clearly surpassed the corresponding whiting glazes in every respect.

Sixth. That the investigation by Mr. Williams and myself pointed to this conclusion, before the dolomite was under consideration.

While I regret that my work was not more conclusive and freer from defects, still I think I can safely say that in the dolomitic limestones of Ohio, we have a very valuable material for glaze manufacture, which will more than replace English whiting, pound for pound, and which will produce glazes of better color and wearing power, and at a marked saving in cost.

The only question I have in mind is whether this material can be obtained in quantity perfectly uniform, from time to time. Nothing but systematic series of analyses will tell this point, but the producers of the lime surely ought to be willing to bear the cost of proving the uniformity of their supply.

DISCUSSION.

The Chair: I am rather surprised at the action of the magnesia in the dolomite, because I have tried magnesium glazes and always found them off-color.

Mr. Cannan: Here are two glazes, one with one-tenth magnesia and the other with one-tenth lime: here is the same thing in dolomite, the same equivalent of lead replaced in each. (Exhibiting samples.)

The Chair: The dolomite is not as good as either of the others?—I speak from this distance, not having examined them closely.

Mr. Cannan: We were burning the tile in a small kiln. Those glazes are workable, though they have a little unfused material along the end, caused sometimes by having them near the flash of the kiln; sometimes due to sulphur scum, The end of the tile which is away from the wall of the kiln does not have it on. But sometimes we find it on the end next to the bag wall, and we attribute it to sulphur.

Mr. Ernest Mayer: You place barium way down on the scale?

Mr. Cannan: We have to do it. We expected to get a high replacement from barium. I have no sample with me to show you our results on barium. We were burning with coal, and found the glaze with 0.05 BaO had little specks all over it, which we attribute to sulphur. We tried it then with charcoal, which certainly is devoid of sulphur, to see if we could do away with it, but did not succeed.

Mr. Mayer: The reason I ask that question is that I think we should go very carefully in making a statement contrary to Professor Seger. I had occasion to use barium once, and came to the same conclusion you have; and I then asked for information as to the truth of placing barium next in the scale of fusibility to lead. I was given the answer that I would find it would come close to the lead, providing we frit the barium before putting it in the mixture. So, because you are using a raw glaze, it is not proper to put it so low down in the scale. I think you will find it a very soft flux, considering the conditions.

In making these experiments you have got here (taking up a white tile) a very open body, and here, (taking up a buff tile) practically a vitrified body, I suppose. Now, you speak of putting the glaze on to the thickness of a sixteenth of an inch. With all those different degrees of vitrification in all these experiments comes the question of crazing, and if you have no other system for getting a certain amount of glaze on these bodies than dipping, do you think it is a fair comparison as to crazing?

Mr. Cannan: I made the question of crazing least important, even down to the fourth place. What I was after, was the most fusible mixture. My idea was to find which material would replace the most lead.

Mr. Mayer: I don't care how good a glaze you have, if you get it on a white body, it might be condemned because it would craze. I was going to suggest to you this: In the manufacture of white ware, we use two kinds of glazes, thick and thin, to catch all kinds of ware. Take a hydrometer, which the dipper watches every few hours, which reads from nought to two thousandths of a section. He can keep that set at 28 to 32 ounces, and keep a very even coat of glaze on it. I mention this, because I do not hear anything of the thickness of glazing in regard to crazing.

Mr. Henry Griffin: Regarding the thickness of the glaze coat, you meant a sixteenth of an inch before it was burnt?

Mr. Cannan: Yes. The idea was to get it as near as the eye would tell us, and sometimes we would use a spatula and cut down through it and try to get it as near the same thickness as we could. At first it would be very thick, and the buff would be the first body dipped. Then we would thin it down a little and dip the red body and lastly thinner still for the white. You understand we are not experts, for it takes a long time to learn to do dipping skillfully. We were not working necessarily for a workable glaze on those particular bodies; we were merely trying to determine the highest possible replacement of lead by the various oxides and latterly by the dolomite. Concerning the barium, my paper starts out with saying we were dealing with *raw lead glazes*. You can tell me that Seger says barium will make a glaze *if it is fritted*. I don't propose to put myself up against Dr. Seger, but I have results on the raw lead glazes with barium which we can't question, nor could Dr. Seger question them.

Mr. Mayer: I was surprised in the same way to find barium very much harder, in fact nowhere near lead at all; and I wrote to our friend Langenbeck of Zanesville for an explanation of the fact that barium was put down as such a soft flux, when I found it so hard. His answer was that barium is a soft flux conditionally; that if I knew enough to read Seger, I would find it must be fritted. After fritting, the barium is soft, just as he said.

Mr. Griffin: Did you find in using barium, a tendency to granulation?

Mr. Mayer: I did.

Mr. Griffin: I found by combining fluorspar with the barium, it seemed to take all that tendency away from it, and I got a good smooth barium glaze.

Mr. Mayer: I think we are all very much obliged to Mr. Cannan for his paper, which is a very valuable one and reflects great credit on him.

The Chair : In regard to dipping, if you will soak all the tile to saturation, and use the glaze very thick, as I did last year, you will get the glaze coating even.

Mr. Herbert A. Wheeler: Do you know whether that dolomite comes from a thick persistent bed ? That analysis is remarkable, and the deposit must be of great value, outside of the field of ceramics.

Professor Edward Orton, Jr. : I do not know anything about the persistency or regularity of the dolomite furnished by the company on whose sample Mr. Cannan was working. When I first suggested this problem to him, I agreed to give him help to the extent of writing to certain manufacturers and get samples of their output for him.

I have been too busy to keep track of what Mr. Cannan has been doing since, and am somewhat surprised at the results he has gotten, as I heard them this afternoon. I assure you I have never heard them before, and I expected rather different results from what he has gotten. I expected a more decided benefit from the use of the dolomite. Regarding the dolomite, there are vast quantities of such dolomitic limestone in Ohio. The half of the state is pretty well overlain with limestone strata, and much of it is nearly pure dolomite. I will prepare and append to this article a note, showing what the resources of Ohio are in the matter of dolomitic limestone. In applying for samples for this use, I did not give a hint as to what it was wanted for, and we got what they are sending out to the trade. As Mr. Cannan says, it would take a series of analyses to decide whether we could get it uniform enough to use for a glaze. We do know that English whiting is pretty expensive, and if we can find this on the market much cheaper, and as Mr. Cannan has found, even capable of pound for pound substitution, I believe it is something of a discovery. But when he gets to the bottom of it, I think it will be found capable of even a larger substitution than pound for pound. Mr. Cannan has shown in some of this work that there was some little advantage in in favor of dolomite; but not nearly what he or I expected.

In regard to what Mr. Mayer said about barium, Mr. Cannan prefaced his paper by stating that he was dealing with *raw* lead glazes. If I can read the German text correctly, where Dr. Seger makes the statement as to the order of fusibility, he makes no qualification as to how it must be prepared. The original statement of Seger is very misleading on that point, for reading that article alone, no one could possibly imagine that fritting was a necessary condition to the truth of the statement.

SUBSEQUENT NOTE, PREPARED BY PROFESSOR EDWARD ORTON, JR.

The question raised in Mr. Cannan's paper, and in the discussion which it aroused, as to the quantity and availability of the limestone or dolomite which would meet the requirements of the pottery industry, has proven unexpectedly easy of demonstration. The essentials which stone must have are (1st) low percentage of iron, (2d) constancy of composition. In the following table are grouped a number of analyses of Ohio dolomites, together with their location. The data were obtained from Vol. VI., Ohio Geol. Survey, 1888:

No.	Name of Producer or Descriptive Matter.	Locality.	Composition			
			Iron and Alumina	Silicious Matter	Carbonate of Lime	Carb. of Magnesia
1	Middle Bed..............	Springfield, Clark Co.	1.80	1.30	55.40	41.48
2	Upper Layers...........	" "	2.70	1.40	53.90	41.90
3	Average	" "	0.56	0.65	54.13	44.37
4	Geo. H. Frey, Middle....	" "	0.20	0.10	54.70	44.93
5	" Upper....	" "	1.00	1.50	54.70	42.37
6	" Average..	" "	0.39	1.55	53.88	43.79
7	Moore & Co	" "	0.42	0.66	53.51	44.96
8	Sintz & Co	" "	0.37	0.68	53.92	44.99
9	John McCarty...........	" "	0.39	0.59	53.77	44.76
10	Holcomb Lime..........	" "	0.54	1.37	53.44	44.49
11	" Building Stone	" "	0.96	7.37	50.13	40.64
12	D. S. Ervin..	Cedarville, Greene Co.	0.57	0.58	53.95	44.62
13	" "	" "	0.53	0.88	53.90	44.58
14	Irvin & Co., Selected ...	Yellow Springs, "	2.00	0.40	54.75	42.23
15	" Average....	" "	0.51	0.72	53.98	44.56
16	" Build'g Stone	" "	0.76	5.65	50.95	42.64
17	" "Blue" Stone	" "	1.40	5 40	51.10	41.12
18	B. Wilson, No. 1.........	Sec. 17 Wayne Twp. Montgomery Co....	0.83	0.80	54.30	43.69
19	" No.2........	Sec. 17 Wayne Twp. Montgomery Co....	0.47	0.39	54.47	44.73

No.	Name of Producer or Descriptive Matter.	Locality.	Composition.			
			Iron and Alumina	Silicious Matter	Carbonate of Lime	Carb. of Magnesia
20	B. Wilson, Rejected Stone	Sec. 17 Wayne Twp, Montgomery Co.	1.13	4.00	51.97	42.07
21	Jos. Rhule	Covington, Miami Co.	0.40	0.85	54.14	44.90
22	Thomas Corey	Brant, Miami Co.	0.40	0.32	54.37	44.01
23	James Dwyer, Upper	New Paris, Preble Co.	0.37	0.61	61.33	37.68
24	“ Lower	“ “	0.38	1.23	67.51	30.84
25	T. J Smith, Lower	“ “	0.56	2.21	67.00	29.26
26	C. Disher, Selected	Lewisburg, “	0.60	0.60	55.20	43.28
27	“ Average	“ “	0.45	0.89	54.58	43.93
28	Wm. Turner	“ “	0.94	1.31	52.70	43.96
29		Sidney, Shelby Co.	1.60	.tr	55.00	42.92
30	Bierly's Quarry	Greenville, Darke Co.	4.60*	....	44.69	50.11
31	Gard's Quarry	“ “	2.20*	....	51.30	45.72
32	Northrop's Quarry	New Madison, “	2.70*	....	51.70	45.26
33	Newman's Caprock	Genoa, Ottawa Co.	0.23	0.04	55.97	44.27
34	Newman's Bottom Rock.	“ “	0.51	0.22	53.04	46.01
35	Wyman & Gregg	“ “	0.16	0.23	54.30	45.14
36	Habbeler Quarry	“ “	0.42	0.28	55.59	43.67
37	Holt's Main Rock	“ “	0.44	0.24	54.61	45.05
38	Kingham's Quarry	Rocky Ridge, “	0.29	0.87	54.10	44.27
89		Limestone, “	0.25	0.29	54.12	44.79
40	H. Rancamp & Co.	Woodv'le Sand'ky Co.	0.39	0.31	53.50	45.79
41	Noble & Co.	Fremont, “	0.27	0.74	54.06	45.20
42	Gottron Bros.	“ “	0.43	0.35	55.65	42.29
43		Fostoria, Seneca Co.	0.28	0.42	56.41	41.98
44	Surface Rock	Bowling Gr., Wood Co	0.40	1.53	53.98	43.25
45	Drillings 210 feet deep	“ “	....	7.76	49.19	40.31
46	Drillings 260 feet deep	“ “	....	0.97	51.62	44.08
47	Wyman, Gregg & Co.	Rising Sun, “	0.63	0.57	53.21	45.37
48	Wyman, Gregg & Co	Lucky Station, “	0.35	0.43	54.20	44.44
49	W. S. Thurston	Bowling Gr., “	0.40	1.53	53.98	43.25
50	Barnd Quarry	Findlay, Hancock Co.	0.39	1.55	53.88	43.79
51	“ Rejected	“ “	1.16	8.00	49.62	40.94
52	G. W. Fetters	Bath Twp., Allen Co.	0.14	1.18	53.69	45.00
53	Norris & Christian	Marion, Marion Co.	0.24	1.60	66.02	33.10

*Iron and Alumina and Silicious Matter.

In the fifty-three analyses given in the above table, there are represented fourteen Counties, in different parts of Ohio, and thirty-seven different quarries or actual locations from which samples were taken. It sixteen cases, more than one sample was taken in one quarry.

The marvellous uniformity of these dolomitic limestones over so large an area is a wonderful thing. In this whole area, representing at least two hundred and fifty miles in distance between extremes, we have the iron and alumina fluctuating only from 2.70 maximum to 0.14 minimum. Only eight cases occur out of fifty, in which the sum of the iron and alumina exceeds 1.00 per cent. In twenty-seven cases, the iron and alumina falls below one-half per cent.

Similarly, the silica is uniformly low. In thirty-one cases out of fifty, it falls below one per cent. The highest silica met in fifty stones is 2.21 per cent., excepting those which are marked as rejected stone, or building stone.

The lime and magnesia in these stones hovers constantly near the typical ratio 56:44, and in three instances, the magnesia exceeds the dolomite ratio.

In view of the above facts, which can be substantiated by a very large amount of additional data if needed, it seems safe to state that Ohio has available an unlimited quantity of very pure dolomite, whose composition is practically uniform over very large areas, and which can be had in the market at a very low cost. If Mr. Cannan's work is borne out by other investigators, and Dolomite is found to be a good economical substitute for whiting as far as the technical considerations go, it is certain that the commercial considerations as to quantity, distribution, cost, purity and reliability will be equally satisfactory.

THE BIRTH OF ENGLISH PORCELAIN.

BY

CHARLES F. BINNS.

Collectors of old china are not infrequently confronted with the statement that a certain specimen has been in the family for hundreds of years. Such a tale is told in good faith, but an investigation will always reveal some error. In the case of Chinese porcelain, rare pieces are found of which the history really covers centuries, but English porcelain was born in 1744, and European porcelain is not many years older.

During the eighteenth century, a tidal wave of porcelain production broke over Europe. The importation of Chinese wares by the Dutch merchants had awakened an appetite for such articles, and the leading men of light and learning began to speculate as to what they were, and how they were made. All sorts of wild tales were current. An old story by an Italian writer stated that "plaster, eggs and shells of marine locusts" entered into the composition, and that the material used was kept buried for at least eighty years, being prepared by one generation for those who were to come.

All these legends were exploded when Böttcher discovered kaolin in his wig, and Europe awoke to the fact that porcelain was being made in Meissen. Between the years 1709 and 1800, upwards of forty porcelain manufactories were opened in Germany, France, Italy, Spain and England. The majority of these were but short lived and nearly all of them were founded by princes and potentates as pastimes and even as playthings. That some succeeded was due more to good luck than good guidance, and those were the times when the practical potter made money. Kings courted and princes bowed down before him, even if his knowledge was of the slightest.

It is not my purpose to trace the history of European porcelain, but to limit this sketch to English soil, relating certain facts which must prove of interest to Ceramists, and extracting from such records as are available some of the technical information which constituted the stock in trade of the potter of that day.

It was in the year 1744 that two men, one described as a merchant, the other as a painter, took out letters patent for the manufacture of porcelain, at Bow near London. One of the ingredients used was a soft earth "the product of the Cherokee nation in America." A second patent taken out in 1749 states that "animals, fossils and vegetables by calcining, grinding and washing are said to produce an insoluble matter, called virgin earth."

The first Bow porcelain was a fritt body, the second contained forty per cent. of bone ash, the nature of which is only thinly veiled in the "virgin earth" just mentioned.

An analysis by Prof. Church of some fragments of this ware, gave the following result:

Silica	40.0
Alumina	16.0
Lime	24.0
Phosphoric Acid	17.3
Magnesia	0.8
Soda	1.3
Potash	0.6
	100.0

From this we are enabled, with some knowledge of the procedure of the time, to deduce certain probabilities. Cornwall stone had not been discovered, feldspar was unknown. The alkalies would be incorporated as a fritt and the silica would be introduced mainly as sea sand. The body could then be nearly reproduced by making a fritt of

Sand	95 parts
Soda Ash	5 "

and then mixing

Fritt	21 parts
Clay	38 "
Bone Ash	41 "

This is, of course, only a rough calculation, but failing the rational analysis of the clay, which probably even the Cherokee nation, though native born Americans, could not supply, we are unable to obtain a much closer result.

It is interesting to note that the territory owned by the Cherokees comprised the states which now produce the best porcelain clay, viz., Kentucky, Tennessee, Alabama, Georgia and the Carolinas. In this the United States may claim to have been the progenitor of English porcelain, even though the parentage was soon repudiated and forgotten.

The procedure at Bow was repeated, *mutatis mutandis,* at Chelsea, on the other side of the Metropolis, but the history of both factories is most fragmentary. Chelsea was chiefly famed for the magnificent colors used. These were undoubtedly made on the works, and served to impart an individuality to the product which is totally lost sight of in this day of the ubiquitous color merchant.

Chelsea porcelain proves upon analysis to be strikingly similar to that of Bow. The knowledge of the virtues of bone ash had become common property, and this substance has held its own ever since as the characteristic ingredient of English porcelains.

It is interesting to compare the formulae of these wares with that of a typical modern English china.

	K_2O	CaO	Al_2O_3	SiO_2	P_2O_5
Bow	0.137	2.737	1.00	4.250	1.090
Chelsea......	0.230	5.200	1.00	7.120	1.500
Modern.......	0.269	2.540	1.00	3.390	1.670

The formulæ have been calculated, taking alumina as unity, for the sake of comparison, and it should be explained that while the figures of the old porcelain are based upon analysis, those of the modern are calculated from a recipe.

The manufactory at Worcester was founded in 1751, and many of the facts of its early history have been laid before the world. There is a special interest attaching to Worcester, in that this is the only one of the old factories which flourishes today.

The Worcester works had a beginning which was remarkable and unique. Certain politicians were desirous of establishing a handicraft of some kind for the purpose of retaining voters in the city, and maintaining the strength of their party at the polls. The bosses were even then active, it would appear, but their activity proved in this case to be a benefit to others besides themselves,

But political wire pulling will not make porcelain, nor were the politicians anxious to establish this particular industry. It so happened, however, that a Worcester doctor named Wall had been experimenting with the view of imitating Chinese porcelain, and had met with considerable success. The inventor and the politicians met, and the scheme was floated.

Dr. Wall's first porcelain was a fritt body, for it was not until some years afterward that bone ash was introduced at Worcester.

Considerable obscurity surrounds the early mixtures, but one of the recipes purporting to be authentic gives:

Sand	120	As the Fritt
Gypsum	7	
Soda	7	
Alum	7	
Salt	14	
Nitre	40	

and for the body

Fritt	76
Whiting	15
Pipe Clay	10

The formula of this fritt would then be

$$\left.\begin{array}{l} 0.523\ Na_2O \\ 0.390\ K_2O \\ 0.087\ CaO \end{array}\right\} 0.01\ Al_2O_3,\ 3.90\ SiO_2$$

$$\overline{1.000}$$

and the equivalent weight 309.

The "pipe clay" would probably be an average English ball clay, such as that of which the rational analysis is given on page 38 of our "Manual." The formula of this clay, taking alumina as unity, is

$$0.026\ K_2O,\ Al_2O_3,\ 2.71\ SiO_2,\ 1.94\ H_2O$$

and the equivalent weight 302.

The whiting may fairly assumed to be pure. The formula of the whole body would then work out thus

$$\left.\begin{array}{l}3.904\ Na_2O\\2.935\ K_2O\\5.277\ CaO\end{array}\right\}1.00\ Al_2O_3,\ 31.43\ SiO_2$$

These figures are startling in their interest. In the first place, it is remarkable that a body with so low a content of clay substance could have been worked at all. Two considerations made this possible — the materials were ground very fine, and the body was worked entirely by hand, often with the assistance of a mucilage.

In the second place, how does it happen that such a blend gained the reputation, as it did, of being as hard as Oriental porcelain? If the formula be recast upon the lines usual for glazes, viz.:

$$\left.\begin{array}{l}0.322\ Na_2O\\0.242\ K_2O\\0.436\ CaO\end{array}\right\}0.082\ Al_2O_3,\ 2.63\ SiO_2$$

it will be seen that many glazes are being used today which are harder than this porcelain body. Where is the secret? I believe that the answer is to be sought in the middle column. The alumina content is the key.

Some of the members may recollect, that in the experiments which formed the subject of my paper last year, it was shown that even very soft lead glazes became practically infusible when the alumina fell below a certain point. Here are the same conditions. Whether from scarcity of clay, or from the poor quality of the pipe clay, or by intentional action, the alumina has been brought to vanishing point and the mass has undergone devitrification. Thus the porcelain body became hard, far harder than a mixture with more clay, and competes successfully with the ancient product of the Chinese.

There is a remarkable analogy between this early product of Worcester and the recent discoveries of Seger.

He has shown that Japanese porcelain is extremely low in alumina and high in alkali, and upon this discovery the composition of Seger porcelain is based. But Dr. Wall has also analysed these Eastern wares so far as the science of his time would permit, and who shall say but that he too had arrived at the correct proportions between the constituent parts of his mix. We have no data upon which to base an estimate of the heat at which this ware was fired. Test cups of the body itself were the only means of control, and these of course tell nothing.

The glaze was a soft compositlon, containing a good deal of lead and no alumina, that is so far as the formula goes. But the silica was largely introduced as sand, so that it is quite likely a proportion of alumina came with it. The formula is

$$\left.\begin{array}{l} 0.101\ Na_2O \\ 0.354\ K_2O \\ 0.545\ PbO \end{array}\right\} 2.07\ SiO_2$$

We have no information as to whether the glaze were fritted or not, but from the presence of soluble salts and from the general use of fritts at the time, it is highly probable that this was the case.

In the early days of porcelain manufacture, but little regard was paid to the cost of production. The art was a monopoly, and as long as secrecy could be preserved, the factory could make its own price, provided the work was good. From the beginning, the Worcester potters aimed at high-class goods. Dr. Wall set a high standard in quality, and he was eminently successful in the production of the dainty tea equipage, which appealed to the æsthetic taste of the wealthy.

Reading between the lines of history, however, it is evident that there were causes of dissatisfaction in the Worcester ware. A glance at the formula tells us that the losses must have been excessive. It could scarcely have been possible to burn such a body to translucency at less than a fifty per cent loss, and no manufactory could long stand up against this.

Search was therefore continually being made for some material which should enable them to avoid the expensive fritting and secure greater stability. Kaolin was unknown in England prior to 1755, and even when it was discovered, it remained a monopoly in the hands of the Plymouth and Bristol factories, but is evident that news of the discovery reached the ears of Dr. Wall, before the substance itself was available, for prospecting tours were undertaken in Cornwall with the view of discovering an independent supply. The result was a find, not of kaolin, but of steatite or soap-stone. The greasy feel of this mineral may have led to the trial being made, but in any case, the steatite was shipped to the factory and used in the formula of the second Worcester body. Here it is:

FRITT.

Sand	10
Calcined Flint	6
Potash	2
Smalt	¼

BODY.

Fritt	5
Flint Glass	5
Crown Glass	2
Hard Soapy Rock	1
Soft Soapy Rock	4½

THE GLAZE.

Double Flint Glass	112
White Lead	35
Foreign China	25
Tin Ashes	4
Potash	4
Smalt	½

I remember once hearing a medical man who had served in Canton, China, giving some account of the Chinese system of medicine. He told of powdered snakes and beetles, of syrup distilled from toads, and other delicacies. His feelings on first learning of these specifics, will now be understood in part by the members of this Society.

The first recipes I had to offer you have been reduced, with no small labor, to intelligible formulæ, but in the present case I weaken. Who shall fathom the mystery of "hard" and "soft soapy rock" of "double flint glass" and of "foreign china"?

That which the recipe fails to do, however, can be done by analysis of the ware itself. Salvetat has given us this, and the formula resolves itself into

$$\left.\begin{matrix} 0.258\ CaO \\ 2.078\ MgO \end{matrix}\right\} 1.00\ Al_2O_3,\ 15.20\ SiO_2.$$

At the same time, it must be confessed that recipe and formula do not march together. Crown glass and flint glass, double or single, could not have been used without some trace of lead and alkalies appearing, and potash is actually used in the fritt.

Shortly after the experiment with steatite, the Cornwall supplies of kaolin and granite became available, and Worcester entered upon the production of bone china, to which the factory has ever since adhered.

The quality which rendered Worcester porcelain the superior of its contemporaries and contributed doubtless to the continuous life of the factory was that of workmanship. A piece of old Worcester is rarely or never seen with a slovenly or imperfect finish.

Not only as regards the manufacture but in the decoration this is apparent, and even in the printed pieces, a moment's comparison with the work of either Liverpool or Caughley will at once demonstrate their inferiority.

Notwithstanding the fact that Worcester possessed neither coal, clay, nor skill, the factory paid its way from the first, and the reason is not far to seek. The beginning was on a modest scale, and nothing but the best work was tolerated. There is always a market for good things, and for those who are able to produce them, and are at the same time content to make small beginnings, there must be a productive future.

How far these principles may meet modern ideas remains to be seen, but in the interests of the porcelain production of America, the idea is put forth.

The discovery of Kaolin stone in Cornwall gave rise to the factories at Plymouth, and Bristol, both of which made hard porcelain, but neither of which was destined to a long life.

Feeling that the members of the Society will prefer the technical to the artistic, I refrain from any comment upon the latter feature of these wares, and offer a series of formulæ comparing the wares, from Plymouth and Bristol with Sevres hard paste and Chinese porcelain.

	K_2O	CaO	Al_2O_3	SiO_2
Bristol	0.0707	0.086	1.00	3.22
Plymouth..........	0.1160	0.000	1.00	3.75
Sevres....	0.1290	0.207	1.00	4.39
Chinese............	0.3333	0.020	1.00	4.72

The extraordinary hardness of the English wares will at once be noticed. Prof. Church has called attention to this and remarks with a melancholy interest upon the fact that when his magnificent collection of porcelain was distroyed by fire, the product of these factories withstood the terrific heat while all the other English porcelains were fused to glassy lumps.

I will invite your attention to one more fact which is that the glaze used at Plymouth was evidently composed of Cornwall stone, with the addition of lime. The formula is:

$$\left.\begin{matrix} 0.484\ K_2O \ldots \\ 0.516\ CaO \ldots \end{matrix}\right\} 0.955\ Al_2O_3,\ 7.64\ SiO_2$$

and, given a sound and undecomposed sample of the granite, this glaze might be well reproduced by 90 parts of stone, and 10 of chalk.

This paper has departed somewhat from the normal lines of our contributions, but there are two reasons for this. The first will be found in the able and, unfortunately, unrecorded address given by our President at Niagara Falls; the second lies in the fact that circumstances have rendered it impossible for me to pursue any original research since the last meeting of the Society.

If it be permissible for me to point the moral of my story, I would sum the matter up in a question.

If it were possible for earnest men in the comparative darkness of the eighteenth century, to establish and perfect a manufacture which required a high degree of technical skill and artistic taste, how much more should the men of the present day, when technical schools and art institutes abound on every hand, devote their energies to the development of all the possibilities of their art and raise their country, their calling and perhaps themselves to a position recognized by the critical opinion of the world?

DISCUSSION.

Vice President Mayer (*occupying the Chair*): You will note the fact that before our friends in England could make porcelain, they had to come to America to get material. I would like to ask one thing about the formulæ: what is this unity of Al_2O_3? I merely ask the question, because I am afraid it may make confusion for us.

Professor Binns: In adapting the chemical formula of the body, I have taken the lines adopted for clay, taking alumina as a unity. You will find it so given in the manual· Professor Orton will explain it to us, I am sure.

Professor Orton: From my point of view, in any compound which we attempt to represent by the molecular formulæ, that element should be taken as a unity, which we deem its essential or characteristic constituent. In a glaze, we take the sum of RO elements, or some single RO element as unity, because it is more convenient to judge of the fusibility, etc., from that standpoint. In a clay, the clay substance is the essential feature of the substance, and it seems therefore proper that its base, the alumina, should be taken as unity. If we were to write the formula of a clay, and take the small traces of alkalies and earths present as unity, we would make a very poorly proportioned formula, in which the alumina would run up to four or five molecules and the silica from fifteen to fifty molecules. In a flux, like iron ore, I take F_2O_3 as unity. In bone ash, I think CaO should be unity, etc. In short, the unity should represent the characteristic feature of the substance, whatever it is. In a clay, Professor Binns has rightly taken Al_2O_3 as the basis, for on that basis only can it be easily compared with kaolin, the type clay.

Professor Binns : I want to call attention once more to the remarkably low content of alumina in the original Worcester body. Take the Bow and Chelsea body; we have the unity of alumina as against 4.250 and 7.120 silica; take the Worcester body, we find unity of Alumina as against 31.43 silica—a very low content comparatively of alumina. Why did that body get the reputation of being as hard as Chinese porcelain? It seems that fact is worthy of note by ceramists, because there is a great deal behind it and I would like to have it brought out.

Mr. Stanley G. Burt : I would like to ask Mr. Binns if he has ever noticed the fact, that in certain glaze formulæ, the addition of the flint does not necessarily render that glaze hard? As we reduce the acid, the mixture actually gets harder and more infusible.

Professor Binns : By reduction of flint?

Mr. Burt : Yes. If you take formula of a Seger cone, and gradually reduce the flint in the mixture, you might imagine at first that you are going to arrive at a constantly lower-fusing body. On the other hand, the flint taken from that mixture makes it practically infusible. The excess of the silica present in the Worcester body does not carry with it the sense of hardness to my mind that it does to Mr. Binns, after considering the fact that it is very low in alumina. In fact the silica actually acts as a flux in certain formulæ. I understood that Mr. Binns said the Worcester body was a hard body, because of the high content of silica.

Mr. Binns : No; I said "due to the low content of alumina."

THE USE OF GLAZED CLAY-WARE AS AN ARCHITECTURAL DECORATION IN EXTERIOR VS. INTERIOR WORK.

BY

OTTO HENSEL.

If you expect anything comprehensive, I must disappoint you at the outset. At present I am able to give but a few disconnected observations on the theme, whose immensity I discovered only on trying to write about it. Everything that is excellent in glazes is embraced, and its mastery requires a good deal of concentrated study.

Nevertheless let a person but start in finding shades for decorative effect, and he is at once confronted by the problem with all its mysteries. Merely to duplicate a glaze is simple enough, yet frequently the problem is to vary existing or find new effects. Frequently the experimenter must ask and answer himself, "What feature makes this glaze valuable for that purpose?" However mean his ability, he has to meet the subject as squarely as the greatest master of the craft. It is necessity spurring him on, over again.

The subject divides itself into decoration of the interior, of the exterior walls, and of the roof. Effects must be studied by the laws of color as laid down in good manuals on chromatics and by those governing the art of building as found in works on architecture. Above all these aids, the student must command a share of good taste and of a healthy critical faculty, and cultivate them. Furthermore, he can only hope to make progress by the study of concrete examples, actual architecture surrounding him. Chicago is a fertile field for such work.

You will perhaps tell me that this is an unpractical scheme leading nowhere; that tastes are as numerous as individuals; and that in practical work, one must give what is called for. Still a study of the truths underlying the art will give the student his bearings, and he is then free to put his cleverness to the test in meeting exigencies of the moment and perhaps bringing a new contribution to the art.

Interior effects in the broadest sense may be taken to include all pottery from porcelain to terra-cotta articles. But then articles of the coarser sort, such as majolica vases, are just as effective out of doors, provided they are are placed on a level with the eye or below it, to enliven a corner of the piazza or of the lawn. Interior effects proper refer to mosaic floors, tiling for walls, and ceiling decoration.

The glaze has a distinct purpose here, according to established laws of chromatics and of architecture. The checkered floor emphasizes the spacousness of the room and the walls give character to it in keeping with its intended use; the ceiling seems to be partly independent of the rest, for its decoration, while depending on the wall, diverges from it.

The effects desired appear to be obtained mainly in three ways, underglaze, which may be strengthened or altered by color in the glaze, colored tin enamels, and transparent or colored glazes over relief, where the varying thickness of the overlying glazes produces light and shade. What is desirable here seems to be either a strong metallic reflection especially suitable for back ground, or a great depth of lustre combined with play of colors. The chemist works in a very complicated fashion, applying freely colored engobes, variously colored clays laid side by side, a great deal of fritt for perfecting lustre, specially prepared colors, and the like.

Decorative effects on the exterior walls are becoming very frequent. Our technical periodicals state that we are entering an epoch in architecture showing a distinctly picturesque tendency; that we demand no neutral tints but bold general effects and harmony of color. I do not venture to state to what extent the effects prevailing for the interior can be utilized here. I have seen little in that line. Several small store-fronts with the narrow strips of walls left beyond the window spaces, laid with richly glazed brick in mosaic pattern, may be classed, I suppose, with what Ruskin styles "commercial art." I saw one church, of broad, tent-like outline and severe style of architecture, of glossy white, though no particularly offensive lustre,—a significant departure. I noted that matt surfaces, flowed smooth for the sake of cleanliness, prevailed. If harmony is brown, as has been stated, I should think it might be used more, provided of course that a suitable attractive glaze is available. Thick and lustrous majolica glazes find a sparing use in outside decoration.

The glazing technique is much simpler. Frequently the chemist can perhaps do without fritts altogether, bringing to aid ground glass and calcium borate, a compound which forms in the raw glaze when quick-lime and boric acid in equivalent amounts are ground with the rest. These glazes are exposed to the changes of temperature the year round and therein lies a difficulty that he who compounds glazes for interior work hardly meets in his channel. Owing to nearness to the observer, the chemist must see that his colors come out clearly and properly in the glaze or englobe, hence some care is necessary in their preparation.

The roof, once decided that the house shall have one, is a separate member in architecture. It does not apologize for being there and can be treated with all the boldness consistent with the plan of the house. Unlike the wall, which may have a dark shady street, or a grove of sombre-looking trees, or an expanse of warm rich nature for background, the roof in our latitudes always has the same background of cold blue. Hence we find suitable either a dark intensely matt surface or warm colors. Thus it suggests massive structure or harmony of color. Owing to its distance from the observer, the coloring must be strong, no half tints. Metallic-looking enamels are out of place here. The gloss of majolica glazes may be employed, but more generally the more matt the effect, the better. It seems to me that of glossy glazes the one that contains the color suspended without being completely opacified, is much more effective than simple undergaze effects.

Although much can be learned from the more complicated departments in the art and chemistry of glazing, the greatest simplicity of procedure prevails here. The effect will be good even with a blotchy color, provided it be strong.

DISCUSSION.

Mr. W. D. Gates: I suppose that among the many things that tend to make the sad life of the terra-cotta man sadder, and makes the nervous prostration that ultimately awaits him almost welcome, none is more appropriate than this question of exterior decoration with glaze and enamel. We have, of course, to furnish our ware as people want it, and there is a growing demand for buildings with enameled fronts, which we have to meet. One of the things which tends to hasten this nervous prostration is the fact of constant haste or pressure. You potters, in making a cup, know that no man is waiting to drink out of that cup. He has something else to drink out of, while you are working. He can use sea shells, tomato-cans and other articles. The sanitary potter need not endanger his bath tubs because a man is waiting; the man can go and dent the surface of the nearest river, if it is necessary. But when a man makes up his mind to use architectural terra-cotta in his building; than our fun begins in earnest. In the West, we have a feverishness in doing things. There is such a thing as time in some places; they have another day, down South. We haven't got it in Chicago—there is only one day. There is always that everlasting rush. If you get tied up in a contract with a forfeiture of five hundred dollars a day, and if you don't have it completed at such a time, you are "it."

Then the question of color comes in, in such a violently aggressive way. The architect has a scheme; he sees in his mind's eye what he wants, and we show him a little sample. (We can all make good samples—I always prided myself that whatever I sent to the building, I could always send good samples to the architect, and I have a poor opinion of a man who can't make good samples). The architect must decide from that little sample, as to what it will look like in large masses; then we must give it to him. Last year, one of our customers had a mania for a blue-white building, even though he had to tear down another building to get it. The next man must have a creamy white, with more life in it. Blue and white was too cold.

A dull, matt surface will not keep as clean as a polished surface, and a man will run the risk of reflected light from large surfaces, the unevenness of which will throw down the light in bucketsful. The dull surface will not do this. The troubles we have with our architect friends keep us moving. I remember straining what few nerves I had left to make a twelve-foot fluted column. I succeeded and then the architect wanted a twenty-five foot column. You often wonder at my sad appearance, and drooping figure, and that is the reason.

The Chair : There is one point I have often wished to speak to architects on, and I wish there were twenty here.

They make, up at Alfred, N. Y., a very good grade of roofing tile. The architect's specifications for one of the buildings at Alfred University insisted that the tile be of uniform color. We have such a roof on one of the buildings now, and the pieces of tile are as near alike one another, as sticks of sealing wax; you can't tell one from another. I think that is a mistake.

I recently had occasion to build a chicken house, and I went out to this roofing tile yard, and said that I wanted some waste tiles and was given what I wanted from a cull-pile. I selected what I wanted and put it on, and I have the prettiest little roof in the country. It gives all the shades of clay product, as it is run from the machine; just such variations as we know in a nice red brick building. We all can recall instances of a brick building painted red, with the lines carefully penciled out in white. I can hardly describe the effect it has on me. It is an absurdity. Clay products should not be absolutely uniform: we lose all the play of light and shade in which lies one of the true values of the clay itself.

So I insisted that I would not put on this uniform red tile. I went to the tile-yard once more and said that I wanted those old tile, instead of the uniform ones which the architect had specified. I found a large stock of those tiles of different shades, and got some brown trimmings, and we now have a mottled roof which is as much superior to plain, one-color roof, as gold is superior to lead. I wish that architects would take our experience into consideration.

Mr. H. B. Skeele : I want to say that our experience bears out very fully what Mr. Binns has said regarding the growing demand for variety in color. Formerly, roofing tile was called for of a uniform shade. Now scarcely one architect in ten will call for a uniform shade. They even go so far that the natural variations of shade of the tile coming from the kiln, which is large, is not sufficient to meet the demands. On one occasion we had to make a special mixture to bring in four different shades quite widely apart, for one order, in order to get this variegated effect in the roof, which is, as has been rightly said, so much more desirable.

THE AMERICAN CERAMIC SOCIETY AND ITS RELATIONS TO THE PRACTICAL POTTER.

BY

EDWARD C. STOVER.

In reading this paper I desire to call the attention of the practical potter, particularly the men who compound the bodies and glazes in our various potteries throughout the country,to the ultimate aims of the American Ceramic Society, which are to improve and facilitate the manufacturing of ceramic products in the United States. In order to do this, one of the duties of our society, is to collect and publish literature on the subject. At its meetings, papers are read and discussion follows on the various subjects of importance and interest. The theoretic, scientific and practical sides are all fully represented.

The practical man is apt to think or say "I care little or nothing for theory, and know little, and care less, for science, but confine my attention exclusively to the practical problems of my business."

Of the practical man who professes the above sentiments, have you ever asked the following questions: What is Theory? What is Science? If not, kindly reflect on the following.

A theory is a conclusion or reason drawn from numerous and accurately observed phenomena, or a deduction of the laws which connect cause with effect.

Science is the recorded experiences and discoveries of mankind, or the knowledge of many, collected and methodically digested, and arranged so as to become easily available.

Every practical man is therefore consciously or unconsciously, more or less of a theorist and scientist; to what extent, predetermines and facilitates the results of his practice.

Every up-to-date and ambitious practical potter should become a member, or associate, of the American Ceramic Society, and attend as many of its meetings as possible, to hear papers read touching the perplexing points in his business, and to enter into the discussions that follow each paper. If the particular points upon which he would like to have a little more light, do not come up, it is then his privilege to bring them up and have them discussed. The exchange of ideas with the members of the society is not only a pleasure, but a permanent benefit to all who hear or read the discussions. It is true that some chaff is always with the wheat in this organization as well as in all others but it is an easy matter, for one skilled in the art, to separate them.

The practical potter in whatever branch of the business, who is making some progress, and expects to keep abreast of the times, should keep in touch with this society, as he would find, from time to time, in its papers and discussions extremely helpful suggestions.

Every practicing potter finds at times the desirability, and frequently the necessity, of making changes in his mixtures, and is required often times to substitute new materials for old, and it is in making these changes by the "rule of thumb" or guessing method, that much vexation and trouble arises. Fortunately the laws of nature are such that, by the assistance of scientific knowledge, much of this trouble can be obviated, and the results desired can be readily figured by simple methods, thereby very materially lessening the experimenting necessary to make the proper change. For example, it is frequently desirable, for obvious reasons, to substitute feldspar for Cornwall Stone. With the assistance of the "Manual of Ceramic Calculation" published last year by our society, any practical man can easily learn how to make the above and other substitutions without materially affecting the constituents of a glaze, in which said change is properly figured and made.

If you are using Cornwall Stone in a glaze, and want to change to feldspar, for every 100 lbs. of Cornwall Stone omitted substitute 36.2 lbs. of feldspar, also add 25 lbs.

China clay and 43 lbs. flint. It is necessary to add both the clay and flint to the mixture, for the amount of spar which will give the same quantity of potash as was contained originally in the stone, does not carry with it the required amount of alumina and silica. The feldspar figured in the above example is orthoclase, or potash feldspar, but any feldspar, or, in fact, any of the materials used, can be readily figured.

A casual glance at the progress of the present meeting, or in fact, any of our former programs, should be a sufficient guarantee to any one interested in Ceramics, and particularly to the ambitious practical potter, to awaken an interest in the work of our Society.

The benefits of increased membership are mutual—the more practical men that attend our meetings, the more interesting, and instructive will our discussions become. On the other hand, the theories advanced on the scientific points brought out will be more apt to be put into practical use, and thus the benefits and influence of our Society will be more quickly and surely felt.

The benefits to every American potter of a general raising of the standard of the quality of ceramic products of this country are so obvious that every thoughtful man must realize that an improved quality means increased demand, and as the standard is raised at home, we shut out the competition from abroad. At the present time we are ahead of the rest of the world in the manufacture of sanitary earthenware, including the heavy line of bath tubs, lavatories, etc. Several very creditable lines of genuine porcelain ware are now being successfully made in the United States. It is our hope that the day is not far distant when the very finest, most desirable, and most artistic ceramic products of every class will be made here, and it was with a view to hastening the time when this standard should be reached, that our charter members formed the American Ceramic Society, and it is with this motive that we invite the attention and co-operation of every practical worker and person interested in Ceramic work.

Each member and associate member in good standing receives printed copies of all papers, including the discussion that follows.

DISCUSSION.

The Chair: Gentlemen, you have heard Mr. Stover's paper. Now is the opportunity for the practical potter. There are a number of them here. The American Ceramic Society is on trial; if anyone wants to criticize it, now is his chance. If anything is being done which ought not to be, or anything should be done, which is being neglected, let us hear of it. Mr. Burt has told us that the makers of the Chatelier pyrometer have sent out inquiries to users of the instrument, asking them to say what was wrong with it; they did not want them to praise it, but to tell them what was wrong with it, so it could be made better. That is our position. Tell us what is wrong with this society and those interested will try to remedy the evil.

Mr. William Burgess: I feel, as a practical potter, that I would like to get a knife into my fellow-potters and stir up those on the outside. I never realized until Mr. Stover was reading his paper what I have been missing. Twenty-four years ago I took my B. S. degree at Princeton. A few years later, I took the M. S. degree, and then went into the pottery business and let it all drop. From that time to this, I have used little of the knowledge I gained then, and if a society of this kind had been in existence then, it would have been everything to me; but now I am as rusty as it is possible to be. And I would like to emphasize the invitation Mr. Stover has extended, especially to the young men starting in the pottery business. I advise them by all means to come in and use the theory and science being developed by the members of this society.

Mr. Yates: I can say, Mr. President, being brought up in the old school, that this seems to be living in a new world. The old familiar "bone, blood and body," we don't hear any more. We have got onto "fluxing material," "body" and "base."

The Chair: The fact is, Mr. Yates, we have got down to the "marrow."

Mr. John W. Walker: This is my first appearance here, and I am here at the instigation of Mr. Stover, and my regret is that I did not know there was such a society before, and have missed what has passed before. I shall become a student of the papers already in the publications of the society. I have enjoyed the meetings, and profited by them, and promise to do my part to the best of my ability as a member.

Mr. Burgess: You inquired for suggestions. All of us have not been educated scientifically and along chemical lines, and I will say that in going over the different formulæ and recipes, instead of using entirely scientific names and terms in the formula, I suggest that you use the common expressions as we know them. For instance, when lime is spoken of, let it be named in the form generally used in pottery. We know what lime is, some of us know what Paris white is, some know other compositions, but we don't all know the chemical term, and I have heard expressions of regret at this meeting that the common names were not used in explanation of the formulæ.

Mr. Stover: I think the point is a good one, and can be carried out without detriment to our literature. I think if we attract the interest of many outsiders, we have got to get down from the H_2O business and say " water." We can put the formulæ in parenthesis. You present that (indicating formula on board) to the practical potter, and he scratches his head and walks away from it.

Mr. Gray: I am glad Mr. Burgess has had the courage to make such a statement. I have been much at sea regarding these chemical formulæ, and I hadn't the hardihood to show my ignorance of it, so I kept very quiet. But I am glad he has been willing to bring it out. I have tried to study it up, but I found the chemical symbols don't always mean the same thing when used by different people.

The Chair: Pardon me, but that is just why we do use them: because they do mean the same thing. When you use the terms China clay and Cornish stone, they don't always mean the same thing.

Mr. Everett Townsend: I don't think the use of chemical formulæ is an obstacle. I have not had any chemistry in school, but just by getting a chemistry and studying it up, I became quite familiar with the terms; and I now like it expressed this way rather than the other way, because I can get my CaO and my Na_2O, etc., out of different materials.

Mr. William D. Gates: I agree with that. When I went to school, I once knew a man who had neglected to learn his Latin grammar, and when he came to read Cæsar, at the boarding house, it was necessary for everyone else to move out. He locked the door, and proceeded to get his Cæsar by main force. My chemical education, like his Latin grammar, was unfortunately neglected, and I have had to proceed with my investigations by main strength as he did with his Cæsar. But I have endeavored, earnestly endeavored, to try to bring myself to the point of my associates here, to try to bring myself to understand the formulæ as expressed, because I think it is the best way to express them. I earnestly intend to try to bring myself to use them that way, and I think others can do the same.

Professor Edward Orton: I would like to ask these gentlemen who have asked us to write our ideas differently, whether they think we prefer to write in hieroglyphics, or whether they think we adopt this method for the purpose of mystifying them. If so, they are most deeply mistaken. We write these formulæ in this manner, because we can convey an idea that way which we cannot convey in pounds and ounces. I defy anybody to take a book of potter's recipes, of which there are several published, and from a study of the body mixtures and glaze mixtures as given in those books, get the philosophy of body and glaze out of it.

If, on the other hand, you take out and assemble from those various different ingredients, the chemical elements of which they are composed (and you will find for the body four or five elements and for glazes about eight—that's all) you can soon begin to see the law and order beginning to emerge from chaos. You who do not deal in CaO, K_2O, etc., have only got to take fifteen minutes, or an hour, to memorize a few of those symbols and you will then be able to use them right along. A man who learns to estimate the value of a glaze or body from the formula, has acquired a method of mental analysis, which can't be obtained in any other manner. We don't do this, because we want to make it hard or uncomfortable for others who haven't learned that way, but because the facts cannot be clearly set forth in any other manner. You must come to this method; not expect us to go back to the old way, which has failed in all these decades to bring any orderly understanding out of ceramic processes and results.

Mr. Burgess : I don't want to be misunderstood in regard to this discussion. I did not suggest this because I think the other way is not better; but in reading the papers or in discussions, it seems to me a little side light might be thrown on the terms in use. For instance, in speaking of calcium you might say, one of the most accessible ways of getting it is from Paris white or whiting—merely giving a passing word to that effect. I have heard this criticism passed since I have been here. What has been said has been intelligible to me, but to others it has not, and I merely suggest a little running explanation for those not versed. Of course, the only way to get to a proper analysis, is through the method you have already adopted.

The Chair : I think this discussion will do good. It will make the position of those using the formulæ clear to those who are not, and perhaps raise more sympathy in the minds of those using them for those who are not. It is a matter which is not new to us. We have threshed it out over and over again, and when we published our manual of ceramic calculation, Professor Orton and I had about the nearest approach to a fight we ever had, about it. I had pretty much the idea announced here today, that we ought to go to the level of those who did not understand chemistry and try to lift them up. He took the ground that we must presuppose some knowledge of chemistry, because it is absolutely impossible in a book like that to put it in any other way; and those who did not have it, must refer to some elementary book where they can get a knowledge of the elements of chemistry.

No man can force these things down yours or others' throats; they will live, if at all, on account of their intrinsic value. Mr. Burgess refers to Paris white as the principal source of lime. We get it from numerous sources, but it is all CaO. It is just because formulae *are simple* that they are used; not because they are complicated. And I counsel those who do not understand them to get right down to it, and learn to understand them, right away. By so doing they will find an enormous accession of power and understanding.

I had a similar experience in my little class at Trenton. I started right in with those formulæ, and Mr. Stover knows that some of them shook their heads; here one dropped out and there another dropped; but some held on; Mr. Townsend, who testified awhile ago, was one that held on, and he is now reaping the advantage of it. What he has done, others may do. As our Secretary says, you had better get right down, and learn them. We could write potash. soda and lime in our formulæ but, *cui bono*? What is to be accomplished? A little study will put you past the need of it. You would be surprised if you would get some of these recipes to which the Secretary alluded, and figure them down to their bones, to find scarcely a pin's point difference between them. They are almost identical.

Mr. Stover: Unfortunately, in this discussion, the fellow you are firing at is not here; and when any of us approaches him, he looks at us with sympathy and says, "No, No," and walks away; and if you give him some of our discussions, it simply confirms his suspicions. I think if you bracket and put in the most common form, the material that is described so the potter who gets hold of that paper can see something he can understand, it will bring him to the meeting; and I believe a practical potter who comes to the meeting, will go home and get an elementary text book and study.

Mr. Yates: I think the remarks of Mr. Stover should be considered by the society. What little I know of the clay business I have acquired from the practical standpoint, and I assure you that the scientific chemist is not the man to draw in the practical man. He has a perfect dread of him, and when he sees him, will say "There's a fellow who is too many for me." If you only draw the men in and get them interested, they will gradually come up to the society. If you discourage them on the start, you will never get them here. Practice makes perfect, and what is familiar to Professors Binns and Orton is simply Latin to the practical man. When I first started some years ago, I got a school book, and started to study it, and you should have heard the ridicule I got from my wife. But I stuck to it, and now I can read the symbols on the board; but I can't compound formulæ as I would like to. I think Mr. Stover's suggestion should be cherished, and the society will grow. Science in the abstract is a very fine thing, but science applied is better.

Mr. Otto Hensel: It seems to me that a former speaker brought out these points. You will find them already published in good manuals, especially in the German. You find all the points mentioned—how such an oxide has a certain influence and produces certain colors. The practical potter finds these things himself, because he is always experimenting. But it seems to me, with all due respect to the practical man, it is a question of the "Mountain coming to Mohammed" or "Mohammed coming to the Mountain."

This Society published a Manual last year, and after looking through it, I think it is a pretty good book. It covers all the points, it seems to me, that a man must know in getting down to this work. It seems to me if the practical man will make some effort, and see what he can do, he will make progress and master the thing all right.

When I left school, I happened to be in a state where the clay-working industry was more advanced than in my home state, so when I left school I traveled around the country to see what I could pick up in practical points. I came to a place where the superintendent of the works had been a mason, and a contractor; he had been put in his present position because he was a good executive, a good manager. But he was always anxious to learn, and he got me to give him some lessons on these very calculations. After only a few evenings work, he had mastered it so he could work it himself.

Mr. Bloomfield: As I understand it, the American Ceramic Society was formed as a means to an end, that end to elevate the ceramic industry of the United States and the world. If, by putting in parenthesis the terms which will explain the chemical formulæ, you thereby enable us to bring in to the society the man with no chemical education, but who is practical; what is the use of keeping that out. I know myself the effect will be that the men who read those books, and essays, will acquire the habit of mind, by reading the formulæ opposite the explanation, which will lead them bye and bye to look out for the primers so they can study those things, and in the natural course of events they will become apt scholars, and we will reach the point for which this society was formed. This society was formed to bring in these men, and elevate them, and if you can elevate them by going out of your way to do it, that is the proper thing to do. The higher up in the world a man is, the lower down he can reach to help a fellow man. Otherwise, what is the use of education.

Mr. Mayer: In answer to Mr. Bloomfield, I will say this society is very anxious to impart all the knowledge it can. I speak individually, as possibly I have not the right to speak for the society, and this is only my opinion gathered from discussions I have heard among members generally. I think the idea of the society is that it is willing to impart all the information it can, but we have got to do it in our own way, which we believe is the best way. It is a very easy matter to familiarize yourselves with these formulæ; very easy, if you think the matter over.

I don't think there will be any very radical change by the society in the method of giving out information. We have got to give it out in the methods universally adopted, not only in this, but other countries, and I think the gentlemen present will see the reasonableness of the ground on which we stand; and if those gentlemen who don't understand this, will spend a little time looking matters up in the chemical books, they will not have much trouble in the future.

Professor Orton: One last word. There has been a great deal said here today about the purposes and aims of the society and much stress has been laid on the altruistic spirit, by which it is actuated. But, it would be wrong to let people think the society was formed wholly for the uplifting of the present generation of clayworkers. If we had only this end in view, it is doubtful if the society could be kept together.

In fact, gentlemen, this society was formed like most others, from motives of distinctly another sort. It was from a desire of educated and trained ceramic workers to get together and discuss freely and in congenial society, the things for which they cannot find sympathetic ears elsewhere; and by exchange of knowledge, to broaden and perfect themselves. When we organized, no such society was in existance in any English speaking country, and I think I may say that its two years of life have very fully realized these objects which I have just enumerated, for all those who have joined it.

I don't mean to say that we had no high-minded unselfish motives in organizing this body. On the contrary, we thought it would be a good thing for the country and for its ceramic industries to have such an organization in it. We thought it would exercise an uplifting influence on clay-workers of all sorts; not directly perhaps, or at once, but in the broad way. Any organization which helps the leaders of an industry to grow perfect in their work, and to enjoy and love their work, helps the rank and file also, in the long run. In my opinion, the way in which we, as an organization, can best advance the interests of the American Ceramic industry, is by diligently laboring to make this society recognized the world over as the most learned and progressive body of men who are working in this field.

How can we attain this result? We meet but seldom and must utilize our minutes, for they swiftly slip away. Shall we spend our time in discussing how to place our facts so simply that people can absorb and assimilate them without working? Who did that for us? Have we not had to labor, hard and patiently, for many years, to get together that which we have? And, has not the struggle which we have gone through been the greatest gain to us, after all? It is because we have struggled and worked and used every opportunity, that we have been able to get such a society into existance. And, it is the struggle which we have been through which makes us able to appreciate such a society.

In my judgment, the greatest good this Society will ever do, will be to set a goal before the American clay worker to which we can inspire him to climb. We should not try to smooth his path too much, or to lift him to this goal; encourage him to climb to it. And, further, the way to accomplish this end, is go on just as we have done, building up to the literature and the science of ceramics. The rising generation will surely come to regard this literature and this science as the foundation stone of their industry. And in time it will accomplish just what we are impatiently desiring to see it do now. Rome was not built in a day. But in a generation, or two, we shall see ceramics lifted from the position of a crude trade up to that of a refined and highly technical art.

Speaking only for myself, I think one of our greatest dangers is a too rapid growth. We do not want to take in members faster than we can make them think and feel as we do on these questions of the policy of the organization, or its ultimate ends. We want members, whenever we can get them, if they are of the right sort. People who believe as we do, whether they are technically trained or not, are a help to us. But I am not in any hurry to get into the Society, a large number of men, who see dimly, if at all, the advantages of maintaining our work on the highest possible plane, and who will expect us to make our proceedings come to their level. Rather, let us take the position which we have so far held; that of maintaining first and highest of all, our own ideals; and second, of offering the right hand of good fellowship to any who, seeing those ideals, believe them good and desire to travel towards them in our company.

"UNWEATHERED vs. WEATHERED CLAY"

BY

EARL McK. OGLE.

In presenting this subject to the society for discussion, I do so to bring out the observations and experiences of the members, rather than to advance any particular belief of my own, although I am free to state that as far as making tile, by the process now in most general use, is concerned, I am in favor of the unweathered material.

The broad statement which we sometimes read, and frequently hear, that all clay products would be better if made from weathered clay, I think is erroneous and entirely misleading. This statement should be qualified by several exceptions, before it can be swallowed without pain, though I am ready to admit that in the manufacture of all fire clay materials, where it is necessary to gain plasticity and to remove as much of the alkalies and contained salts as possible, it does improve the texture of the clay to have all the materials thoroughly weathered. This is also true of many classes of pottery, where the materials are merely mixed in a semiwet state and pugged, but where all the materials are thoroughly mixed in the slip tubs and are then lawned, dried, ground and repressed, I believe that the advantage lies with the unweathered clay, and that it produces a body more uniform in texture and color than the weathered clays.

It is of course recognized, that all clays do not exist in the same condition or formation, and therefore each clay or bed of clay may require a different manipulation, but I am of the opinion that nearly every clay that can be slipped or washed up to a creamy state, will produce better results when unweathered than when weathered. In using the word *weathered*, I mean *thoroughly weathered* clay; not a pile of clay fifty feet square, that has been exposed to the weather for a year, and which is frequently called weathered clay, when as a matter of fact not over eighteen inches or two feet of the exposed surface could possibly be affected in a year's time.

My reasons for believing so strongly in unweathered clays are as follows:—when clays are taken fresh from the pit, they frequently contain pyrites, lime, alkalies—etc., which upon prolonged exposure are decomposed, and washed out, or by decomposition stain the entire mass, whereas if they are immediately washed, lawned and dried, only a slight per cent. of the alkalies or fluxing materials are removed, and the various minerals in the form of sulphates will be removed entirely when passing over the lawns. This would therefore tend to give a more uniform mass than could be obtained by the weathering process, and if we are to depend upon an analysis for compounding our bodies (the analysis being usually made from the bank or pit or unweathered clay) we would likely be lead astray in using the same formula, after the clays were weathered.

Quite recently to settle the matter for myself, I used a common buff clay, which many people had told me it was impossible to use without weathering. I took the pains to make two tile which are herewith exhibited, one from clay fresh from the bank, on the back of which you will find marked "New," the other from clay which had been thoroughly weathered for considerably over a year. In order that the conditions might be exactly the same in handling both clays, they were washed up separately at the same time, lawned through the same size screen or mesh at the same time, dried the same number of hours side by side, were ground to the same fineness, were pressed with exactly the same pressure at the same time, being made in a double die so that there could be no possibility of any unevenness in pressure. They were then fired side by side in the kiln, and there seems to be from a causal observation no apparent difference in either of the samples, but I think that the unweathered clay makes slightly the best tile. The white marks on the face of this tile, are due to the fact that on the platnum there adhered some of the clay of another color which was being used at the time that the trials were pressed, so of course these marks should not be taken into consideration.

I have tried the same experiment with red, salmon and chocolate clays and in every case the tile made from unweathered clay was generally of a more compact texture and better finish than those made from weathered clay.

The uses of clay are so varied, as well as the formation of the beds of clay, and every pottery uses a different process in the manipulation, that it is not possible to lay down any general rule that is applicable to all, but I am confident if this subject were more fully investigated and these experiments or trials compared, that a considerable saving could be effected to our manufacturers as well as to improve the quality of the articles produced.

In conversation with many people on this subject, the opinion seems to be that the same or better results could be obtained if the clays were slipped and lawned immediately on being taken from the pit, but that the expense would add considerably to the cost of manufacture.

While I have no figures to present as a basis of calculation, I am of the opinion that the first cost of slipping is not perceptibly greater than the cost of weathering, and when the further fact is considered that a larger per cent. of first class goods could be made from the clays so treated, I believe that the slipping process would be cheaper in the end, for the greater part of the manufacture of clay products is not so much in the cost of the material, or the first manipulation of same, but is due largely to the loss in the kilns, and in imperfect pieces, so that in fact it seems to me poor economy and a very short-sighted policy to try and save a few cents in the cost of manipulating the raw material, to lose dollars from the effects of same in the finished product.

As to the question of ageing or allowing the clays to rot after being blended into a body, this is an entirely different proposition, and I believe that it is now conceded that this does improve the character of the manufactured articles. The principal reason advanced by the French ceramists for this, is that the organic matters contained in all clays, when subjected to ageing conditions in a damp place or in the presence of water, are decomposed or undergo a sort of putrefaction. The evolution of gas which takes place while fermentation is going on, causes a movement or readjustment of all the atoms or particles contained, and therefore produces more plasticity and homogeneousness, and this causes the body to shrink less and more regularly in the fire.

The organic matters which exist in the clay, are destroyed by spontaneous combustion under the influence of moist air; this at the same time reacts on some traces of sulphates which are found there; and changes them into sulphuretted hydrogen, while changing into carbonates at the expense of the surrounding carbonic acid. Salvetat's explanation of this is, that the sulphuretted hydrogen developed in this process is formed by the sulphate of lime changing into sulphuret of calcium under the influence of certain organic matters, and escapes when this sulphuret comes in contact with the carbonic acid in the air.

The above however does not disprove that weathered clays are better than unweathered clays, but on the contrary, I believe tends to confirm the position I take, that where clays are washed on removal from the pit, they produce a better and more uniform class of articles than the weathered, and my reason for so believing, is, that washing and lawning through a proper mesh will remove the larger per cent. of organic matters contained, as it is a well known fact that the refuse caught on the lawns, contain a very large per cent. of organic matter; this, if allowed to weather, would have decomposed to such an extent that a greater part of it would have passed through the lawns, and been incorporated into the articles made therefrom. When it is taken into consideration that even a hair will cause very serious flaws in fine pottery, we can see to what an effect organic matters, if contained in any quantity, would produce. It only remains to be determined whether the weathered clays contain less organic matter than the unweathered clay, which is thoroughly slipped. This I have not been able to demonstrate on the account of lack of time, as well as the delicate mechanism necessary, but I hope and trust that some member of the Society will be interested enough in the subject to do so, and present the matter to the Society with a synopsis of the tests at some future meeting.

DISCUSSION.

Mr. Stanley G. Burt: Mr. Ogle's paper touches on a topic on which I had it in mind to say a few words a little later.

Mr. Ogle divides the topic, as I would have done, into two heads, weathering and ageing of clay. I had intended to consider the weathering, but I agree largely with what he says on that subject; but in the ageing of the clay, Seger attributes the consequent development of plasticity, to a different cause. He attributes it to the development of acidity in the clay. He took clay and mixed it and allowed it to settle: took the same clay and added a little alkali to it; and in a third sample, he added a little acid. And he shows that the one to which he has added the acid, has increased in plasticity, and the one to which the alkali was added has lost plasticity. And he argues that the traces of alkalinity which the clay originally carried is overcome by the acids generated by organic decomposition; and that is one of the main features obtained in ageing.

We, in this country, do not as a rule worry ourselves about the ageing of clay. When I was in Europe, I noticed that the ageing was considered of the greatest importance. The clay is stored in cellars and kept at least six months. In the Royal Berlin Porcelain Works, there was a magnificent long cellar, where the clay was piled up four feet high, and they never think of touching it under six months.

The Chair: Is that filter-pressed clay?

Mr. Burt: Yes.

The Chair: Has it been pugged or rolled?

Mr. Burt: No, it is piled up just as it come from the press. Mr. Ogle to my mind correctly divides the subject into two heads weathering and ageing. I agree with him that there are many cases in which weathering is not only unnecessary, but absolutely injurious. It is on the other half of the subject, however, that I wish to ask a question, that is in reference to ageing. Now the European potters age their clay bodies, that is let their clay mixes stand in damp cellars for months. As far as I could find out, the great object of this was to increase their plasticity. This it undoubtedly does. Seger has explained to us that this result is due to the generation of acid in the decomposition of the organic matter present. What I am anxious to find out is what is there to be gained besides plasticity in ageing a body. The mixes I am speaking of have been carefully washed, lawned and pressed.

Mr. Edward C. Stover: Professor Hart, of Lafayette College, has examined weathered and unweathered clays from the standpoint of bacteria. He claims it is a case of fermentation, in the cellars where the clays are ageing; and regarding this statement which Mr. Burt gives us, about the acid and alkali, wouldn't the acid generated simply increase the tendency to fermentation?

Mr. Ernest Mayer: When a man tries to make out that unweathered clays are better than weathered clays, he has got a contract. There are some classes of clay ware, that we cannot make without weathering and ageing the clay. I do not think it is right to assume that the case is proven, by the means of these two samples. Unfortunately Mr. Ogle is not here, so we cannot ask for more details about these two samples. There ought to be more trials made, before coming to any conclusion.

The clays from which vitreous china is made, all stand in cellars, and I don't think those wares could be made without ageing. There are some factories in East Liverpool being built now, with magnificent cellars, for the clays must be held sometime; the longer, the better.

Mr. Alfred Yates: It depends largely from what standpoint of clayworking you argue. Referring to plastic flooring tiles, I would say, that clays ought to be finely ground and weathered before pugging.

Plastic flooring tiles made of weathered clays are made superior to those made of newly ground and pugged clays. In surfacing flooring tiles with slip, better results, are obtained by using aged slip, it gives a smoother surface, and better finish. By using new slip the coating will be irregular and there will be more or less cracking on the face, showing more shrinkage in one part than another and much rougher finish than produced with aged slip. Therefore, ageing slips and weathering clays in my opinion is a decided improvement for this kind of ware.

Mr. H. B. Skeele: It may be interesting to the members of the society to know that in the manufacture of roofing tiles at the extensive Broseley tileries in Shropshire, England, they do not use any clay that is not a year old. I saw them handling clay, a year's supply at a time, on a large platform on which the clay was lying about 5 ft. in depth. A two-inch water pipe ran the full length of this platform above the middle of it, and from this the clay was kept constantly wet. At the end of six months they turned it over by hand, throwing unweathered lumps to the top and after six months' more treatment they considered the clay ready for use. They seem to think they can't make good ware out of clay that is not handled in this way.

Mr. Yates: From experience I have had in making shingle tiles in Staffordshire, England, I will say that it is impossible to make as good tiles from new stock as from weathered clays.

Mr. Thomas Gray: I recently had a little experience along this line of ageing clay. We had some clay on hand for six months which was finally made up in ware. This ware was carried in the unburnt condition in a wagon over cobble stones for about two miles, and we only lost fourteen pieces. The man who made the ware said if they had been made with the clay taken from the press, as we always use it, it would not have stood the trip without considerable loss. But the clay was very tough—he had worked the clay himself, and he said it was aged. He spoke of it as being a very tough, nice clay to work with, and very much improved by ageing.

Mr. Cannan: I think the dry press process of making floor tile cannot be compared to the manufacture of plastic ware. When you slip a tile, you don't care for plasticity, and I think this discussion is not taking into account the differences in the process of manufacture.

Professor Orton: This is a very large topic which is being discussed. There are subjects for four or five papers in it. One point which I think ought not to be confused with others, is the effect of weathering on breaking down the fossil structure of the older clays. Anyone who will go into the face of a shale bed, and make some paving brick from the crop clay and then work under the hill where it is unaltered by weathering and make some more brick by the same process as before, will find out the difference. The weathering has done something to the clay, which cannot be duplicated without time and trouble. That is one point to develop which properly, would require a paper of considerable length.

Another point which has been touched on a little, is the development of objectionable impurities in the clay during ageing—the conversion of sulphides into sulphates, which are soluble. There is much to be said pro and con on that point. There is no doubt that it does take place. If you are going to use a slip process at the conclusion of the weathering, by which the bulk of the sulphates are taken into solution and removed, you get a final material practically free from sulphates. I think the weathering under such circumstances is questionably advantageous. If, on the other hand, you are going to work a material by a process which will give no opportunity to wash out the soluble substances, it is well to work it as quickly as you can after mining, before weathering sets in, and expel the sulphur from the sulphides by roasting it out in the kiln.

A third subject of inquiry is the effect of the washing process on the plasticity of a clay. Every stone ware potter will tell you that he can " throw " clay after a pugging process, but not immediately after slipping and filter pressing. If they attempt to throw the clay, or turn it from the pugged material, just as it comes from the filter press, it lacks coherence and toughness to a remarkable degree. The throwers always insist on the filter-press plates being partially dried out, and then throw into the tempering pan, and reground with a little more water, and so brought up to a tough condition. If this is not done, they say that each grain appears to remain independent of its neighboring grains, instead of forming with them a fibrous or cohesive whole.

Then the point which Mr Stover brought up, is a subject about which more than one paper, could be written without exhausting this field for speculation. I refer to this theory of plasticity being influenced by bacterial growth. Some may scout this idea, but I do not. I believe there is a great deal in it. If I am not mistaken, that matter was discussed before us before. All of us have plenty of instances of a slip set away for a short time becoming sour and offensive. It contains nothing but the ingredients of the clay suspended in water. The water is not especially impure, yet there is something either in the clay, or the water, or in both, that furnishes material for fermentation and decay. These changes are accomplished with the usual signs of fermentation and decay, viz.: change of color and bad odors. Yet unquestionably there is an increase of strength and toughness in the clay resulting from this treatment. Take the glass-pot makers; they prepare their clay one or two years in advance. They keep beds of plastic pugged clay from four to eight feet thick, over the whole bottom floor of their factory buildings. It is so short when first pugged, that they could not use it; but after aging a year or two, it develops a fibrous structure and great toughness. But, when it is aged, it smells offensively. I believe bacterial growth has taken place, which has rearranged the grains of the ingredients, and permitted them to come together, as they did not do when forcibly compacted in the filter press; and the new structure is desirable, where the old structure is not.

REPORT OF THE COMMITTEE ON SEGER CONES.

Mr. President and Members of the Society:

One year ago, you appointed a committee of which I was designated as chairman, to take up the subject of certain alleged defects in the behavior of the lower members of the series of Seger cones, or to be more accurate, that portion of the series in which oxide of iron is a constituent (No. 3 to No. 010). These defects had been pointed out in a paper by our Mr. E. E. Gorton (in Volume II, pp. 60-73), and substantiated in part, by the experience of various members of the society, who discussed the paper. The committee was instructed to look into the subject, and report back its findings to the society.

The committee has, for various reasons, been unable to meet as a body, though some communication has been had by mail, and some exchange of samples for observation and analysis. I have personally been able to give the subject some little attention, and in the absence of a formal report of actual committee work, I submit the results of my observations as follows:

THE USE OF IRON AS A FLUX IN THE CONE SERIES.

Mr. Gorton's paper claimed the iron in the cones was the cause of numerous irregularities, and thought its use was a grave mistake.

I can fully agree with the opinion given that the use of iron in the cones is objectionable, and there can be no doubt that the fusibility of these cones would be affected, depending on whether a reducing or oxidizing fire is maintained. Outside of this, I do not see any objection, nor do I believe that the other defects cited are due to the iron.

In an experiment I made on this point I replaced the iron with oxide of zinc, leaving all the other materials the same, and making all allowances, but these cones failed in the same way as those described in the original paper.

ON THE CHANGE OF THE CONES COMPOSITION BY ACTION OF THE KILN GASES.

From the description given of the irregularities of the cones, such as drying up, and bending at the base instead of gradually bending, and afterwards from examination of the cones themselves, I was fully convinced that a part of the trouble at least was caused by the volatilization of the boric acid, as I had stated in the discussion of Mr. Gorton's paper. From the statements given by him, that such failures as described did not occur when burning in either a small muffle kiln or an open kiln, and also that no other persons present had had a similar experience except that which I gave in my discussion of the subject, and from the fact that some of the kilns which Mr. Gorton was using were the same as some of our own, led me to believe that the cause for the trouble with the cones lay in some peculiar condition in the large muffle or terra cotta kilns.

In kilns such as described in the original paper, the heating is done by conduction and radiation, and this is necessarily slow, and, as the muffles are seldom tight, a portion of the combustion gases find their way in. This passage of kiln gas through the muffle would not be objectionable were it not for one fact, and that occurs when burning fuel containing sulphur. In an examination of the vapor and gases from a kiln, it was found that these were distinctly acid and it was here that the trouble was located. In another experiment to determine what action the acid vapors had on the cones, it was proven that the fritt, which enters into the composition of the cones, was attacked and partially decomposed. In a laboratory experiment, it was found that sulphuric acid of a strength as low as .05 per cent. readily decomposed a part of the fritt.

It will readily be seen that as soon as a decomposition of this kind occurs, especially at a low temperature, and in the presence of moisture, there is no reason why some of the boric acid should not be carried away.

Another material affected by the acid vapors is the calcium carbonate, the greater part of which is converted into calcium sulphate, and this, in combination with the loss of the boric acid, seems to my mind, to fully explain the failure of the cones.

From an examination of some cones Mr. Gates sent me, I found the tips to be burned to a hard mass, with little evidence of vitrification, corresponding to the dry coating of the exterior. The base of the cones had melted, but only in the interior, showing plainly that this part had not been affected, and giving evidence that the action was a slow one, working from the surface inward. Naturally the tips or smallest part would be most affected.

It will be seen on reference to Mr. Gorton's paper that he had overcome the difficulty in his own case, by dropping the iron from the cone batch and adding an increased proportion of a boric acid fritt. Cones made in this manner, fired in the same kiln as before, went down in a slow gradual curve from top to bottom, and did not grow hard on the tips or surface and did not swell at the base.

If my theory that the loss of B_2O_3 by exposure of the glaze to an acid-impregnated atmosphere is correct, it may be asked, why did not the cones with the additional boric acid show the same or more pronounced defects.

If we will remember the action that takes place in the kiln, the evaporation of the water from the ware, and also from the kiln itself, and the combination of this water with the acid gases from the combustion products, then we have these acid vapors constantly at work slowly decomposing the fritt, and as it is decomposed, some of the boric acid is carried off.

From many analyses made on material where this action has taken place, it was invariably proven that there was a shortage of boric acid.

But it was also found, that where the vapors attacked the cone mixtures, that a protective coating was built on the exterior of the cone, consisting of alumina, silica and calcium sulphate, which protected the cone from further action of the acid vapors.

In another experiment, taking the cone mixtures of Mr. Gorton and subjecting them in thin layers, or as a glaze, to the action of the kiln gases, it was found that they failed to mature, that is, the lower layers fused to a perfect glass, while the top was dull, dried and blistered. On examining the last mentioned glazes it was found that they contained over two and one-half per cent. of SO_3 which of course would be combined with the calcium.

Again looking at another side of the question we have a frit of low melting point and by increasing the frit and dropping the iron, chemical combination will begin to take place at a lower temperature.

But in the cones containing the oxide of iron we have a mixture with less of the fritt, but with a mass of finely divided sesquioxide of iron, which separates the particles, and has a tendency to keep the cone open, and in a porous condition, and which is one of the last elements to enter into combination, thus giving every opportunity for the acid to attack the fritt and the lime.

To prove the matter in another way, we took the same samples before mentioned, and after firing them in the same kilns with fuel containing little sulphur (for instance oil) and with wares which have not a large percentage of non-oxidized sulphur, none of the troubles spoken of took place, but instead all glazes and cones matured in their proper way and were bright and clear.

My conclusions therefore are :

First. That in the Cremer cones or those in which a boro-silicate fritt is used, the oxide of iron introduces an element of uncertainty into the mixture which is unnecessary and that it can well be dropped.

Second. That the oxide of iron is not in itself the cause of the irregularities in the behavior of the Cremer cones, when used in muffle terra cotta kilns.

Third. That the real cause of the difficulties given is the expulsion of boric acid from the cones by sulphuric acid generated in fuel gases and also the conversion of calcium carbonate into calcium sulphate in the same manner. By these two changes, the ratio of fluxing ingredients is very seriously interfered with, so that the cone no longer melts when it should, nor melts in a normal manner when it finally does go down.

I therefore recommend the use of some other frit, which will not be so readily attacked by acid vapors, and that either little or no free lime be used in the construction of the Cremer cone series.

Respectfully submitted,
A. F. HOTTINGER,
Chairman Committee.

DISCUSSION.

Mr. Stanley G. Burt: I would like to ask Mr. Hottinger whether he has analyzed a fired cone, to determine what per cent. of boracic acid was lost?

Mr. Hottinger: I can not give it by figures. I never made any direct boracic acid determinations, but I could tell about what the loss was.

Mr. Burt: You showed a clear loss from the cone ?

Mr. Hottinger: Yes, a clear loss.

Mr. Edward C. Stover: That loss of boracic acid in the cone, may possibly account for some changes in boracic-acid fritt-glazes. We would have the same results under the same conditions as you did with the cones, and that would seem to lay the blame on the kiln gases for some of these defects.

Mr. Burt: I think undoubtedly a good deal of probability is added to Mr. Hottinger's theory of the volatilization of the boracic acid, from the fact that this effect is not noted where we have a rapid fire. If we have a rapid fire carried to the conclusion, the cone appears to behave very well. But in the long, prolonged fire, we get the true opportunity for the volatilization and probably get that effect.

Now, regarding the suggestion of adopting an entirely new type of cone. This present cone, undoubtedly, is bad. The aim of the committee should be to get something better. There is no question, but that the Cremer cone is a miserable mixture. Those of us who are using a lower fusing glaze, have right there the means of making a cone, which would serve them very well. We have a glaze maturing at say, 05: It is comparatively easy to advance to say 01. And I would be glad to know whether the committee has gone further with the work and is ready to offer a substitute cone ?

Mr. H. B. Skeele: It may be of interest in regard to this question to state simply that we are burning our kilns with oil, burning off in forty-eight hours, using cone No. 08. It melts out in the middle, and runs out at the bottom, sometimes standing reasonably upright in the middle.

Mr. Alfred Yates: I would like to ask some information, being brought up in the old school and accustomed to using trial pieces. I would like to hear what is the advantage of the use of cones over trial pieces ?

Mr. Ernest Mayer: I believe one of the greatest advantages to be derived from the use of the cone is this. I do not suppose there is a man here who has not some time had trouble with his kilns. The fireman knows all about its going wrong before you do, if he knows his business. One of the first questions you ask the man is, "Where are your trials?" He has them all ready for you—You don't know whether they are one day, or five years old, or whether they are all out of one trial hole. He will say, "I don't know what the trouble is—here are the trials I fired by." He can't do that with the cones. There they are, and they tell their own story, whatever it is. He can't fix them: he can't take them out, or put them in: he can't get at them. That is one of the greatest advantages.

The Chair: He can knock them over, if they won't go down.

Mr. Mayer: I had a fireman once who, if the cone did not go over when he thought it should, would shove it over with the poker. But it did not make a particle of difference, for they would always tell their own tale anyway. (Illustrates on blackboard.) A cone which goes over naturally will go over something like this (in a curved shape); but a cone which has been shoved over, will have a break in it right there (indicating), which will show that it has been shoved over every time. It will bend nearly at right angles. I charged my man with it, and he admitted having shoved them over several times; but there is nothing about this that will fool anybody twice. If it is shoved over, it always breaks it; but if it comes down by the fire, it will come down gradually like that (indicating). There is no question about that.

Mr. William D. Gates: Suppose the cones begin to get too hot: can't a fireman regulate that to a slight extent by letting in cold air?

Mr. Mayer: I suppose it could be done to a certain extent.

Mr. Thomas Gray: I differ with Mr. Mayer as to the cracking of a cone which is pushed over. I have showed Professor Orton one of his cones, which was not pulled over, but which was cracked.

Mr. Mayer: I did not say it would not crack except by being shoved over. That very often happens. In making up the cones, if there is the least tiny crack in a cone, it will magnify in the burning and become worse, and many of them do that. But the appearance of a cone which has been pushed over is entirely different to any which have come down by the fire. I have used them for a little over five years, and have never had a mishap in any way with them.

Mr. Burt : There is another point in favor of the cones. As I understand the trial piece system, the ordinary method is to take a ring and spread it with the glaze ; and during the progress of the burn, you draw out that ring, and endeavor by estimating how the fusing is taking place in the glaze on that ring, to judge the character of the fire of the kiln. I have used that test for years, tested it and used it, and it is after all a mere question of guess work. Once fused, there is nothing to tell whether you have gone too far, or whether you have an over-fired kiln. With the system of firing by cones, you say, " Cone so-and-so represents my heat"—say cone one. You put in cone one, and also put in cone two. If cone two comes down, you know the ware is spoiled. How can you know that with trials. The glaze may show well up to four or five cones above the proper heat. We know many glazes, which mature at cone 05, which are beautiful to cone 5, but the ware which bears them will be injured. In firing with slip trials, it is purely a question of judgment. One man will say, "Here we have a fairly brown color on that, just about the shade of color we generally fire to." Some one else interested in the firing may come along and he thinks it is not quite what is wanted. It is only a question of guessing as to whether you have the proper shade. With the cone, you know it is down and what that means. There is ground for argument in the former case, but no room for argument as to when the cone is down.

Mr. Lemon Parker: I want to know whether fifteen or twenty per cent. more or less of silica in the different classes of clays will have any effect on the behavior of the cones? I mean the material in the kiln itself.

Professor Edward Orton, Jr.: If I understand your question, I would answer that the cone is picked out in reference to the ware you are going to burn, and it responds to the same law the ware does; in both, the process is one of silicate fusion. You select a cone which fuses at a temperature where your ware is in such a state of vitrification as you desire. It may be porous or vitrified. Different persons use cones for different ends. The cone measures the *heat-work*, and that is what you want to know. You do not care about *temperature* as such; you want to know what the temperature has accomplished in the way of changes in the clay body.

The cone is nothing more than a modified and improved trial piece. It is an empirical method, not exactly a scientific one, but it tells you better than any scientific instrument yet invented, what conditions you have in your kiln. It is influenced by the same agencies that the ware responds to, and tells more closely when a given result is attained than any mere temperature instrument,

For Mr. Yates' benefit, I will say that Dr. Zimmer read a paper at the first meeting on this subject, and the paper, together with the discussion which followed, were published in Volume I of our transactions. And in Volume II, you will find this question pretty thoroughly gone over once more as to the advantages and the disadvantages of the cone system.

Someone said here this morning that the principal difficulty is likely to be found in the members of the series below cone four, where iron or boracic acid is used. I think the statement is probably true, that the principal difficulties are found there. Though I make and sell cones, and it might be thought to my advantage to keep their difficiencies out of sight, I will say that I believe that the truth is more profitable than anything else, and I welcome these discussions because they always bring out some point not hitherto noticed.

I have had repeated instances where complaints have been made of cones failing to melt at the proper point. In one case, which was submitted to me only about two weeks after the last meeting, I had in mind the suggestion which Mr. Hottinger made last year as to the influence of sulphuric acid, and looked for it. The cones sent to me were numbers nine and ten, and it was stated that they had come down, side by side. They were blubbered and blistered so that we could not tell which was which. The user complained that the cones showed no distinction as to melting point, and that his ware was injured on account of it. I sent them to the laboratory and had them analyzed, and found *two and one-half percent of sulphur*. I also had found on inquiry that a muffle kiln had been used. I had studied the operations of a muffle kiln on other occasions, and I had observed the extremely strong smell of sulphur in the atmosphere of the muffle, as compared with the smell of the gases proceeding from an ordinary kiln.

The atmosphere in the muffle is not subject to the same lively movement as in an ordinary kiln; consequently the ware fired in a muffle is to a certain extent *soaked* in the muffle atmosphere, and if this atmosphere contains more sulphuric acid than usual, it is natural that wares containing free calcium carbonate will take it in, and their melting point be affected. I think, therefore, that where muffle kilns are used, and cones are used, a distinct effort must be made to ventilate the muffle, even at the expense of the fuel used.

Dull or blistered glazes are likely to result from the same causes as do dried up and swelled cones. It seems to me a ventilated muffle is almost necessary if we expect to get good results. This is one difficulty in the use of cones, but it is a point which the user, and not the maker, must overcome.

In regard to cracking, Mr. Mayer, who makes and sells cones in the open market, just as I do, says that cracking is always likely to take place. One or two gentlemen here have alluded to the fact that they have had difficulty in that way. So far, I have had the most trouble in keeping my cones free from cracking, in those numbers in which the proportion of clay is lowest. Cone four requires very little clay. As we go up to twenty, we get more and more clay in each number, and have to use less and less binding material, to make the cone solid and strong. From four to nought two or nought three, the amount of clay used is very small, and we must depend wholly on artificial binding material to hold them together, and it is very difficult to prevent air-checking where this is done. If checking does occur, the cones are liable to crack open in the firing. But it is easy to tell the difference between a cone which goes down out of its place by cracking, and one which goes down in the normal way. If it goes down by cracking, it goes down quick and at an angle. Part of it will be straight, very frequently. Whereas, if it goes down in the normal way, the curve will be slow and gentle.

The fact will remain, when the last word pro and con has been said on Seger cones, that no other form of heat-measure has yet been invented, which will tell so exactly what you are accomplishing in your burning. It is an invaluable adjunct to every ceramic process, but it ought not to be used to the exclusion of other means of heat measurement. I have never advocated giving up the old trial pieces or rejecting the Le Chatelier pyrometer,—both have uses. If you can afford to buy a pyrometer, use it by all means; but the cones will still give you information, which you can not get from the pyrometer.

Mr. Parker : If we have magnesite brick on one side of the kiln and fire brick on the other, would not that affect the behavior of the cone?

Professor Orton: The fact that you have got a basic brick on one side of a cone and an acid brick on the other side, would not affect the cone itself.

Mr. Parker: Aren't the cones of a silicious nature and does not the gas carry basic matter? The reason I asked the question is that I have noticed when we had silicious fire brick in the kiln with magnesite brick, one would be glazed from the vapors thrown off by the other. We would find the fire brick glazed, while the magnesite did not seem to be affected.

Professor Orton: If you will look up an article written by Dr. Cramer on the "relative effect of the fluxes," and published in the Thon Industrie Zeitung for 1896 or 1897, you will find that he shows that almost every ingredient entering into ceramic compositions is capable of appreciable volatilization, and he could even show by analysis the amount of each element lost by firing.

Mr. Parker: Your cones are silicious, are they not?

Professor Orton: Yes; but I do not think that the losses by volatilization would be so great as to affect the temperature at which the body of the cone melts.

Mr. Parker: The cones do get coated, but I don't know whether the coating affects the heat at which they fuse or not.

Mr. Bloomfield: Some years ago, I thought I would do a little experimenting with a small kiln, making magnesite brick. I made some magnesite brick, and burnt them in a kiln (not knowing any better) lined with ordinary fire brick made in New Jersey. I did not have any lining left to that kiln. The magnesite destroyed the clay brick, and I don't see why it would not affect the cones.

Mr. Burt: I think in that case the volatilized matter would settle on the face of the cone, and we would have possibly a slight trace of vitrification there, which would seal the inner part of the cone from the approach of the kiln gas. I am anxious, however, to see the society return to my original question which has been lost sight of, as to whether it is within the province of this committee to suggest and offer some substitute for the lower cones, and whether it has some suggestion along that line.

Mr. Hottinger: I was somewhat in doubt as to what the committee was expected to do, or should do, in the matter of suggesting a new cone-series. I have made one or two suggestions, to use a more stable fritt in making the cones, and to eliminate the free or easily decomposable lime.

Mr. William D. Gates: I want to urge that this committee act further. Unfortunately I did not hear all the paper; but there certainly should be something done by the Association regarding the lower numbers of the cones. It seems to me Mr. Hottinger has been left too much alone in this work. Of the cones sent to us to burn under coal-burning conditions, only a few of them were burned; the rest were put upon the shelf when we returned to burning oil, and did not get burned.

There is no question about the value of cones. There is nothing but what a man can doctor in a kiln, but he can do less with cones than anything lese. We have been left in the position where we use trials, cones and pyrometer; and the burner, if the pyrometer did not act as he thought it should, and if the cones did not act as they should, has been very much inclined to go back to what he thought best, the old reliable trials. But that is because of the difficulty with these lower cones. We cannot tie absolutely to one thing. I do not know just exactly what is to be done in this matter, but I had great hopes that something would be accomplished by this committee. I reasoned that if the sulphur was the damaging agent, and would affect the cones, it would also affect the glaze, but I have never followed this out.

Mr. Burt: I would like to make a motion. I move that the report be received and the committee be continued, and the Chair empowered to appoint additional members if necessary, and the committee asked to continue the work and offer a substitute for the Cremer cones. (Seconded)

The Chair: Just one remark, before I put that motion. We don't want to get in the habit of organized committee work, to the exclusion of individual work. Professor Orton made appropriate remarks on this subject twelve months ago, to the effect that when a line of this sort opened for investigation, it was in order for each member to appoint himself a committee of one, to follow that investigation; and I think that is the right policy to follow. A man investigating on his own account is very much more at liberty than one who has to consult a colleague. We all stand ready to render each other assistance when it is wanted, and if one of us has an idea, he can follow it out better alone than if he has to go hand in hand with somebody else. I put that to you as my view. I don't want to refuse to put the motion, but I think it wiser to let it stand that any man is at liberty to investigate along these lines if he is disposed to do so.

Mr. Burt: I want to say what has been said before on that subject by Mr. Stover,—'what is every man's business is no man's business'; whereas if a committee is entrusted with an investigation of this sort, it is still empowered to call on other members for their experience and help in the work, and thus we will arrive at something.

Mr. Mayer: The sentiment that what is everybody's business is nobody's business is strictly according to my idea of the situation. If the old committee is continued and the chair asks somebody else to act with it, I believe the work will be done and a report made to this society at the next meeting.

The Chair: I am very glad to have elicited these points of opinion. It has been moved and seconded that the committee be continued and the chair have power to add to its number, and that its report be presented at some future time, preferably at the next meeting.

(Motion adopted.)

Mr. Skeele: I want to call attention to the fact that Mr. Burt's motion called for the development of a new series of the lower number of cones by this committee. I hope this portion of Mr. Burt's motion will not be overlooked.

FURTHER NOTES ON THE DISTRIBUTION OF COST IN THE MANUFACTURE OF WHITE-WARE.

BY

ERNEST MAYER.

Last year I gave the Society my ideas as to a method of determining and distributing the cost in the manufacture of white ware, by the use of which a potter could intelligently figure the cost of any piece of ware he has to offer for sale, and also determine what articles are most profitable to sell, and what articles to avoid as far as possible. The first paper was complete as far as it went, and gave the method by which I have been able to trace out the cost in my own works.

This year, I desire to present a more careful analysis of the various sources of cost in manufacturing white table-ware. It is given solely for the purpose of showing the final distribution, the amounts in dollars and cents that the various sub-divisions of labor and material actually reach in a years business.

I believe such calculations are useful in many ways; they will certainly tell their own tale; I will indicate by a few examples the ways in which I think it of use. Suppose a man in locating a pottery, having first decided the probable out-put, wants to know at what advantage or disadvantage he will be, in a new location away from the recognized pottery centres. Knowing from these figures, the amount of coal, clay, &c., on which he will have to pay freight, he will be able to tell exactly where he will stand.

This calculation will also show the cost of production; which items are the principal ones to attack, with a view to making more economical production, either in material or labor, and in the question of the application of the machinery to the industry, the figures given will show exactly the amount that can be saved; provided the capacity of the machine is known in advance. A careful study of these figures will reveal much other information that it is impossible to indicate here. I am certain of one thing, viz: that any pottery manufacturer who will take the trouble to work up a similarly exact table of cost distribution for his own plant, will find that he knows vastly more about his plant and his business when he gets through than he does now, and in my own case, this information has many times over repaid the labor spent in getting the facts together. No one can tell where the leaks in any industrial enterprise are, until he has followed them out by methods like this or which attain the same general end.

The following tables have been prepared as representing the cost of manufacture of the following quantity of white ware.

40000	dozens	handled tea-cups
40000	“	tea-saucers
40000	“	seven-inch plates
40000	“	six-inch plates
40000	“	five-inch plates
40000	“	four-inch fruit saucers
40000	“	individual butter-dishes
6666	“	covered dishes, eight-inch
3333	“	open dishes, eight-inch
3333	“	“ dishes, twelve-inch
3333	“	bakers, eight-inch
3333	“	pickle dishes
3333	“	boats
3333	“	covered butter dishes
3333	“	sugar bowls
3333	“	jugs—42
3333	“	bowls—30
316666		

or 40000 complete sets of assorted table-ware

Purpose for Which Expense was Incurred.		Amount in Dollars	Percent of Total Cost
Making	Potter	18016.00	12.3950%
	Mould Runner	1680.00	1.1558
	Batter out	1796.00	1.2356
	Finisher	4852.00	3.3381
	Handler	1080.00	.7430
	Handle maker	428.00	.2944
	Use of machinery (Pugging)	372.00	.2599
Clay	Material	12204.00	8.3963
	Stain	804.00	.5531
	Press cloths and lawns	224.00	.1541
	Labor	2232.00	1.5356
Moulds	Plaster of Paris	700.00	.4816
	Labor	1504.00	1.0347
Biscuit Firing	Placing	3132.00	2.1548
	Drawing	1440.00	.9907
	Fireman	452.00	.3109
	Oddman	1144.00	.7870
	Wad Clay	228.00	.1568
	Fuel	2964.00	2.0392
	Sand	64.00	.0440
Biscuit Saggers	Labor	420.00	.2889
	Clay	348.00	.2394
	Fuel (drying)	32.00	.0220
	Wash	4.00	.0027
	Washing (labor)	32.00	.0220
Biscuit Loss	Biscuit Loss	1716.00	1.1806
Glaze	Material	3668.00	2.5235
	Labor	376.00	.2586
	Fuel	72.00	.0495
	Grinding	296.00	.2036
Glost Firing	Placing	5616.00	3.8638
	Drawing	2076.00	1.4283
	Fireman	868.00	.5971
	Oddwork	1156.00	.7954
	Fuel	3048.00	2.0970
	Wad Clay	280.00	.1926
	Glost warehouse, dressing, selecting and putting away	2124.00	1.4613
	Biscuit warehouse, brushing and stamping	2684.00	1.8466
	Dipping	1988.00	1.3677
	Dippers' help	852.00	.5861
	Looking out ware for packer, including foreman of glost warehouse	1940.00	1.3347
Amount forward		84,912.00	48.4220

Purpose for Which Expense was Incurred.		Amount in Dollars	Percent of Total Cost
	Amount brought forward	84,912.00	48.4220
Glost Saggers ..	Labor	1924.00	1.3237
	Clay	1020.00	.7017
	Fuel (drying)	84.00	.0578
	Sagger wash	244.00	.1679
	Bitstone	100.00	.0688
	Labor washing	180.00	.1238
Stilts and Pins.	Stilts and pins	920.00	.6329
Dead Expenses	Fire insurance	1408.00	.9687
	Boiler insurance	28.00	.0192
	Taxes	340.00	.2339
	Water	100.00	.0688
	Steam heat	792.00	.5449
	Lighting, natural gas	184.00	.0578
	Lighting, electricity	8.00	.0055
	Tool account	1344.00	.9247
	Repair account	2648.00	1.8218
	Office and management	1300.00	7.7744
	Watchman and foreman	2796.00	1.9236
	Expense account, advertising, etc.	1476.00	1.0155
	Sundry supplies	276.00	.1900
	Stationary	768.00	.5284
	Employers liability insurance	132.00	.0908
	Modelling	1320.00	.9081
Printing...	Labor—Printer	9760.00	6.7148
	Labor—Girls	13440.00	9.2467
	Engraving	2200.00	1.5136
	Color	1068.00	.7347
	Oil	200.00	.1376
	Paper	680.00	.4678
Hardening On...	Labor, placing, drawing, preparing-kiln, coaling, and firing, kindling and wheeling out ashes	2060.00	1.4173
	Bits	132.00	.0908
	Fuel	1608.00	1.1063
	Total	145352.00	100.0043%

This quantity of ware will, after deducting the biscuit loss, loss in printing, hardening-on kiln, dipping, and kiln shed, produce in the glost warehouse, on an average, as follows:

78%=29640 sets of 100 pieces each, run of kiln
20%= 7600 sets of 100 pieces each, thirds
2%= 760 sets of 100 pieces each, lump ware and loss
38000 sets

In the above calculations, the principal materials used in the manufacture of this ware were found as follows:

Flint	440 tons
Feldspar	175 tons
China clay	410 tons
Ball clay	205 tons
Cobalt	400 pounds
Coal for biscuit, glost and hardening-on	3810 tons
Plaster of Paris	450 barrels
Wad clay	125 tons
Sagger clay (biscuit and glost)	342 tons
Borax	14 tons
Lead	8.5 tons
Whiting	10 tons
Printing color	800 pounds
Printing oil	88 gallons
The total cost of which is about	$31,960.00

Classifying the expenses in the above table, I find the following grand divisions:

Cost of labor	70.7721%
Cost of materials	21.3081%
Management and miscellaneous	7.7231%
	100.0043

FURTHER NOTES ON THE TENSILE STRENGTH OF CLAY MIXTURES.

BY

EDWARD ORTON, JR., E. M.

In Volume II of the proceedings of this society, pp. 100-125, an article was published by myself " On the relation between the tensile strength of clay mixtures and the size of grain of their non-plastic constituents, " in which it was shown that in the experiments made, the tensile strength of the clay mixture varied inversely with the diameter of the grain of the sand, between the limits of .04 and .002 inch. But, another fact was also brought out, viz., that where a much finer sand was used, that a remarkable loss in strength occurred, which could only be explained on the assumption that the drying of the brickettes containing the fine sand had been too rapid, and had permitted cracks to be formed, by which the strength of the clay mixture had been reduced.

Obviously, such a supposition requires demonstration before it can be accepted, and the following work was done to see whether we could show by further experiment, that our hypothesis was correct.

Not having enough sand of the No. 6 size, the diameter of whose grains averaged 0.00017 niche, the first task was to make a sufficient supply. Mr. Frank H. Morrison of Denver, Colorado, was set at this work, and he prepared about 15 pounds of sand by the same general system described in the previous article. There being no intermediate sizes required, he proceeded directly to his goal, first floating out a 3 minute sediment, from repeated quantities of the sand, and next sedimenting this 3 minute material and refloating it for periods of 20, 18, 16, 14, 12, 10, 10, 10 and 10 minutes respectively. Thus no particle was used, which had not floated in relatively still water for 10 minutes and a large proportion of the sand had floated for much longer periods.

This slushy impalpable material was then sedimented, collected in jars, treated with acid to remove metallic iron dust, of which there was considerable, even in this immensely fine pulp, then dried, screened to break up the caking formed by drying, and was measured by the microscope. The average diameter of the grains were 0.00024 inch. This is .00007 inch coarser than last year's No. 6 sand. Some of this difference is probably in the microscopic measurement, but the second batch of No. 6 sand was probably coarser than the first, as it was prepared by a shorter and more direct process.

The next step was to design a series of tests whose results should clear up the matter of this anomalous behavior of the clay mixtures containing very fine sand. Accordingly a large batch of the clay mixture, No. 6, containing 50 per cent. Mayfield ball clay, and 50 per cent. sand No. 6, was prepared by blending in the slip state, evaporating to dryness with frequent stirring, pulverizing and screening the dried residuum, and tempering the screened powder to molding consistency with water and vigorous wedging.

This mass of clay was now made up into brickettes. The process of manufacture was in all respects the same as heretofore, and Mr. Morrison made every brickette with the most punctilious care.

The brickettes were divided as fast as made among five separate series, whose treatment in drying was varied as follows:

Series No. 1: Dried in the laboratory, at a temperature of 75° to 80° F., in the open, and without precautions of any sort. Time of drying, nominally six days, but they were really dry long before they were tested.

Series No. 2: Dried in the same room, but was covered with a heavy thick cloth which was quite damp when first applied and which became dry only after 36 or 48 hours. Even then, it shielded the damp brickettes beneath from air currents and delayed their drying somewhat. Time of drying, six days, at which point they were tested.

Series No. 3: Dried in a closed box, about 18″ x 16″ x 8″ deep. The brickettes were placed in the box when quite fresh, and the lid was then nailed tight down and the air volume inside left to exchange its moisture with the outside air through the minute cracks in the box and pores of the wood. Time of drying, two weeks.

Series No. 4: These brickettes were placed in a box of the same dimensions, which had been previously washed, till the wood was well dampened. This box was then closed tightly, covered with a wet cloth and placed in a damp cool place and left to dry out slowly. The wet cloth and damp box would certainly delay the beginning of the drying of the brickettes for a considerable period. The box was kept in underground cellar for about three weeks or possibly four, until the brickettes had become solid and practically ceased to shrink. The box was then placed in the coolest corner of the laboratory and after another period was finally opened and its contents tested after eight weeks drying.

Series 5: These brickettes were placed on shelves in an air tight porcelain jar, provided with a lid which tightened down upon a rubber gasket by screw pressure. In the bottom of the jar was a mass of granular chloride of calcium, one of the most powerful dessicating salts obtainable. The brickettes did not touch the salt or come close to it. The lid being made tight, the jar was set in an underground vault and kept cool for eight weeks, during which time, the moisture of the air in the jar was rapidly taken up by the salt, but the drying must have been very slow, since the heat for vaporization of the water still in the clay, could only have been slowly furnished in this cool dark place. Examination of the brickettes from week to week showed them to be slowly progressing; they could be dented with the finger after four weeks drying. They were finally dried and tested at eight weeks, after having become perfectly solid and long past shrinkage.

Each series was finished by heating from three to seven hours in a hot air oven, at a temperature of from 120° to 130° C. This was done to expel hygroscopic moisture and to bring each series to a uniform condition. Mr. Morrison, who performed all the work so far described, found that the temperature at which the brickettes were finally broken was important: that a brickette was weaker at 100° C than at room temperature, or 30° C, and distinctly weaker if allowed to thoroughly cool and take up a little hygroscopic moisture again.

Series No. 4 and 5 showed a vesicular structure in a marked degree. The probable cause was organic fermentation or decay, which was favored by the long quiet exposure to dampness. Three or four weeks certainly elapsed before any of these brickettes had become reasonably solid, and in this time, these processes probably fed on the organic matter of clay and water and generated the minute gas cells which caused the porosity noticed in the resultant brickettes. Series 1, 2 and 3 did not show this structure at all, but were made from the same lump of clay at the same time.

The series were broken in exactly the same manner and on the same machine as was used in last year's work, with the following results:

Series No. 1. Dried without precautions.

Brickettes—	1	188.41	5	192.51
"	2	118.78	6	172.03
"	3	159.74	7	184.32
"	4	204.80	8	155.64

Average—182.49 pounds per square inch. Fluctuation, excluding No. 2, which contained a large flaw, was 26.9%.

Series No. 2. Dried under a cloth.

Brickettes—	1	172.03	5	167.93
"	2	176.12	6	184.32
"	3	192.51	7	208.89
"	4	135.16	8	188.41

Average—178.17 pounds per square inch. Fluctuation, 40.8%.

Series No. 3. Dried in a tight dry box.

Brickettes—	1	188.41	5	188.41
"	2	192.51	6	159.74
"	3	143.36	7	163.84
"	4	172.03	8	200.74

Average—176.13. Fluctuation, 32.6%.

Series No. 4. Dried in wet box, in eight weeks.

Brickettes—1....114.68	5....221.18
" 2....172.07	6....225.28
" 3 ...184.32	7....221.18
" 4....217.08	8 ...192.51

Average, (rejecting No. 1, which was broken while too hot to handle, while all the others were permitted to cool down,) 204.80. Fluctuation, 26.0%.

Series No. 5. Dried over chloride of calcium, in cool spot, in eight weeks.

Brickettes—1. ..143.36	5....212.99
" 2....163.84	6....237.56
" 3....163.84	7....229.37
" 4....204.80	8....225.28

Rejecting No. 1, which was also broken hot, the average is 205.53. Fluctuation, 35.8%.

Collecting these results, we have

Series No. 1—Quickest, severest drying	182.49	pounds,	T. S.
" No. 2—Somewhat slower..................	178.17	"	"
" No. 3—Still slower	176.13	"	"
" No. 4—Very slow indeed..................	204.80	"	"
" No. 5—Artificial conditions...............	205.53	"	"

The maximum strength developed was 237.56 pounds per square inch, by the last process.

Comparing these results with last year's figures on the brickettes of corresponding compostion we find:

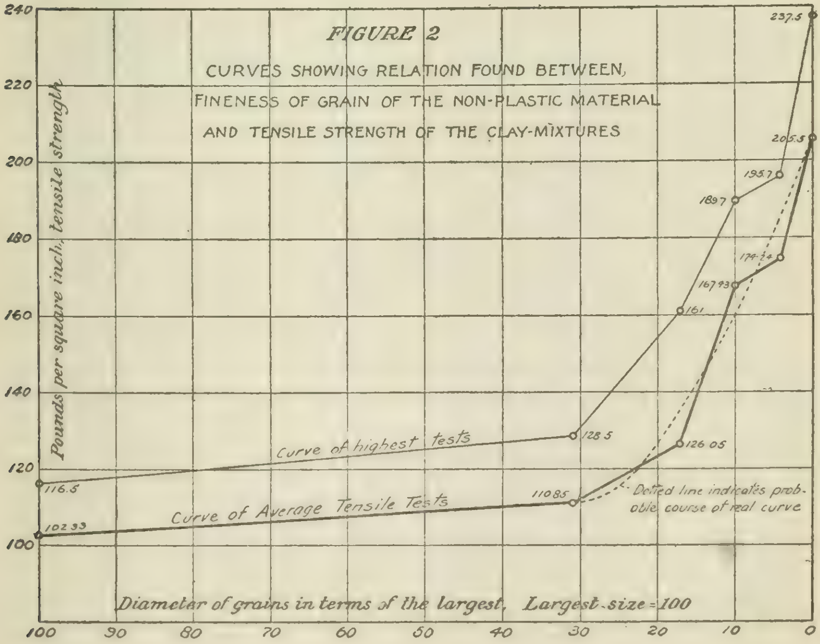
First. The lowest average of any of the five sets of brickettes is higher than the average of the brickettes made of coarser grained materials, viz.: 174.24 to 176.13. This proves that last year's work was abnormal, in some respect which cannot now be determined.

Second. The highest average of any of the five sets, which certainly cannot have developed any more perfect structure or greater tensile strength than the clay can be made to give in actual conditions, if the occasion justifies, is very materially above the last year tests on sand No. 5, viz., 174.24 to 205.53. This tends to prove the assumption given, viz., that faulty drying may materially alter the tensile strength of a clay brickette and that drying is therefore a feature which must be rigidly specified in any statement of methods of determining tensile strength, and must be rigidly observed by all workers who expect their results to bear comparison with others.

Third. Repeating for the sake of convenience the results of last year's series, as amended by the fuller knowledge of No. 6 gained by this year's tests, we have the following:

Number of Mixture	Average Diameter of Grains of Non-Plastic	Average Tensile Strength
1	0.0364 inches	102.33
2	0.0114 "	110.85
3	0.0064 "	126.05
4	0.0038 "	167.43
5	0.0016 "	174.24
6	0.0002 "	205.53

The curve of last year, redrawn to show this new fact is given herewith.



Fourth. We may now safely amend our general conclusion of last year as follows:

The logical deduction from all of the data here presented and so far obtained, is that the tensile strength of mixtures of a plastic ball clay with equal quantities of non-plastic sands, will vary inversely with the diameter of the grains of the sand, from grains of 0.04 inch down to the finest sizes obtainable.

Fifth. Shorn of the obstacle of last year's work, and having now largely overcome the inconsistencies we then found, we may now believe, until it is shown to be untrue:

That the non-plastic ingredients of a clay influence its tensile strength inversely as the diameter of their grains, and fine grained clays will, other things being equal, possess the greatest tensile strength.

DISCUSSIONS ON ANNOUNCED TOPICS.

Defects of the Continuous Kiln of Today and Prospective Improvements.

Mr. J. Parker Fiske: After we have been soaring in the clouds with the chemist, in the beautiful discussions we have been enjoying on these technical matters, it occurs to me that when we come to consider such a homely, humble subject as continuous kilns, we may be brought down to earth with the proverbial "dull and sickening thud." However, I have no apology to make for proposing the subject now before you, because I believe, as I think all do, that the subject of kilns and burning is one of the most important practical subjects which can be brought up for discussion.

We know that the American manufacturer of ceramic ware is leading the world today in many departments. In the manipulation of his clay, in the forming of his clay into ware, and especially in the drying of that ware, he is certainly the peer, if not the leader of the world. I was much impressed with what Mr. Mayer said with reference to the English manufacturer who referred the Australian to America for the latest and best practice. It was certainly a wonderful tribute to what has been accomplished in America. At the same time, I think you will agree with me that in the matter of burning our ware, we are not on a plane with the practice of Europe, particularly that of the Germans. It is in the burning of our ware today that we are farthest behind. In the burning of our ware, we are not only subjected to a too great expense, but also to a great proportionate loss of our ware.

In looking over the possibility of improvement in this line, I find we have four types of kiln, viz., up-draft, down-draft, the so-called tunnel kiln, and the continuous kiln. Up-draft and down-draft kilns have been brought nearly to perfection. We can hope for little improvement in these lines. The tunnel kiln has never been brought to a satisfactory condition, so far as I know. Therefore, it is to the continuous kiln that we must look for any great improvement in burning.

I have been so much impressed with the importance of this subject, that I have devoted much time to the investigation and study of the subject in the last two years, not from the standpoint of an operator of the continuous kiln, but rather from the standpoint of one who has observed what other people are doing. I have been in position so that I could travel and visit almost every continuous kiln in operation in the eastern part of the United States, many of them two or three times. I have been very hospitably and kindly treated by those operating them, and they have given me the fullest data as to all important facts connected with their operation. In investigating those kilns, I have been impressed by several important points.

The first is, the great diversity in design among them. They have the Hoffman type, the so-called chamber kiln, differing somewhat from the Hoffman, and some others which cannot be classified. Some of these kilns are nearly a total failure, some are moderately successful and some are very successful. It must be said, however, that on studying results produced in these kilns, we are not nearly up to the plane of the German continuous kiln-user and burner; and the question has often been raised as to why it is that the continuous kiln has not been successful in America, while it is so well established in Europe, that not only common brick can be burned successfully, but front brick, terra cotta, roofing tile, and even high grade porcelain. In talking with one of our new associates, Mr. Voelcker, I was surprised and much impressed with the statement that they are burning Miessen porcelain in the continuous kiln.

It is rather hard to answer this question satisfactorily, but there are few facts I would like to bring to your attention. In the first place, the American clayworker has not devoted himself to the study of the continuous kiln. He has been contented to invite his foreign co-workers in ceramic work to come here with their designs, and build kilns for him according to their own ideas; unfortunately, he generally has mingled a few of his own ideas with the foreign ideas, which may perhaps account for some of the failures. In America, we want to do things on a larger scale, more rapidly and more cheaply than anywhere else in the world. This perhaps accounts for much of our progress in manufacturing. But in regard to continuous kilns, I think it is one explanation of our frequent failures. Many of those kilns have been built too large, too quickly, and much too cheaply.

There are a few points which we can state as positive defects of the continuous kiln of America, and which have been largely overcome in Germany, as I understand from considerable correspondence with German kiln builders which I have had and from some reading on the subject. One defect of the continuous kiln in America is they have built the burning chamber too large—too high and too wide—so that we get a too great variation of heat between the top and bottom and front and back of the chamber. I was very much impressed with the articles Mr. Ashby wrote in "Brick" a year or two ago, giving some dimensions of English kilns, in which the chambers were very small, indeed; something we know little about here.

Another serious defect of the continuous kiln in America is the fact that the fuel almost invariably is distributed among the ware, which may be satisfactory in burning common brick, but will never be satisfactory for anything else. There is only one kiln in the eastern part of the United States in which the fuel does not come in contact with the ware. I refer to the Guthrie kiln at Union Furnace, Ohio. I am sorry Mr. Lovejoy is not here to speak to us on that subject.

Another serious defect of the continuous kiln in America is the fact that we try to work the kiln too hard. We try to get too much out of it. Instead of being willing to carry away waste gases from the fire when absorbing a portion of the water smoke from the bricks in advance of the fire, we try to carry the waste gases so far through the green ware that it must condense in acid dew on the brick, thereby causing scum.

Another serious defect on the continuous kiln in America, which, so far as I know, has never been corrected here, but which has been avoided abroad, is the almost invariable use of underground flues. The underground flue is very bad in connection with any kiln, but in connection with the continuous kiln it is particularly objectionable, because we are dealing with comparatively cool gases; it is almost impossible to keep the underground flue dry and warm, so the draft will be good. It generally produces a poor and variable draft.

Another serious defect is that, in every kiln in this country, I believe, we take the water smoke from the bottom of our kiln, thereby carrying the condensing moisture to that portion of the kiln which contains the bricks which are sustaining the weight of the ware above them; and that is prolific of much of the trouble we have in continuous kilns.

It was with a great deal of pleasure that I have made the acquaintance yesterday of our new member, Mr. Voelcher, who has newly come from Germany with the very best German practice on the matter of continuous kilns, and from him I certainly expect great improvement in kiln building. I hope he will speak to us later on this subject.

I believe in speaking of the improvements which the continuous kiln must undergo, I may state a few as follows. We must build our kilns smaller, the chambers lower and narrower, so we can get more uniform heat from one part of the chamber to the other; we must build our kilns of a higher grade of construction, putting into them not necessarily better materials, but putting the materials together better, so they will be more permanent, and so they will be absolutely air tight, which is one of the essential features of continuous kilns and which has not received proper attention heretofore. I believe, also, we must be content to work the continuous kiln to a less degree, taking away the gases from the kiln somewhat warmer, before they have a chance to condense on the green ware. I believe we must take the gases away from the top of the chamber instead of the bottom, in order that the lower portions of the way may not be injured, particularly when subjected to a great weight of material on top of it.

There is another point which I think of great importance, but which has not yet been touched upon. I believe the continuous kiln must be erected by men properly trained for that work. I do not believe it will ever prove successful to allow the kiln to be built by people inexperienced in this class of building, even if they have the proper plans and specifications. The great success of the down draft kiln today, is due to the fact that the building has been in the hands of business men, who thoroughly understand their business, and undertake the building of these kilns as a business proposition under guaranty as to what the kiln will perform, with forfeiture clauses if it does not perform it. Unfortunately the continuons kiln has not been put forward by that class of men in this country, as a rule; consequently, there is not the confidence in that class of kiln which there is in up and down draft kilns.

I am a firm believer in it. I believe it is the coming kiln of America, as it is at present the most successful kiln of Europe; and I believe it is perfectly right for this society, for that reason, to devote a portion of time to the discussion of this question, to find out what are its defects today and what should be its improvements in the future. And if a society like this will give the subject such consideration, I believe the continuous kiln in America will be brought to the plane of the foreign kiln, and bring us as clay-workers to the front, not only in the manipulation and drying of clay, but in the burning of it; and thereby place us in the lead of the entire ceramic industry.

Mr. Alfred Yates: The object of using the continuous kiln in the past has been the economy of fuel. That object is undoubtedly accomplished by the horizontal draft continuous kiln. The principal thing for which we should strive at the present time is to maintain this economy of fuel, and at the same time improve the quality of the ware burned in continuous kilns. I think the kiln of the future is the *down-draft* continuous kiln, in which we can burn fine goods and still maintain the same low cost in burning. The original continuous kiln was invented for the purpose of burning low grade or "backing up" bricks.

Mr. Voelcker: I understand that Mr. Yates has said the continuous kiln was used for burning "backing up" bricks. He is in error there. They can burn the finest pottery in this continuous kiln; they can make no better anywhere.

Mr. Yates: I had an experience of some eighteen months with a kiln, said to be an improvement on the Hoffman kiln. If you want to burn with a reducing flame, you have got to work on the down-draft principle. Whether economy can be maintained in the continuous kiln, maintaining the quality of the ware, is a question for the future. I don't know of any continuous kiln burning nice face brick, or pottery, as I left England twenty years ago. It was not done there at that time.

The Chair: That seems to be just the point; on this side of the ocean they do not have that success; and on that side, they do.

Mr. Fiske: We are after concrete, scientific facts, not generalities. All must admit that the continuous kiln is a success abroad, and it has not been a success in this country; although I wish to be understood as saying that there are some quite successful continuous kilns here, and when we consider those burning common building brick, there are quite a large number of successful kilns. But for high-grade ware, porcelain, etc., we know there is nothing burnt in this country in that class of kilns. I think it would be interesting for Mr. Voelcker to tell us the distinct differences between the continuous kilns in this country, built after the Hoffman type, and the kind used in Germany to burn Miessen ware.

Mr. Cannan: I would like to say that Mr. Ellis Lovejoy, at Union Furnace, is burning face brick, in continuous kilns, and there is no doubt that he is making a success of it. He makes no distinction whether he burns in his down-drafts, or in his continuous kilns. For my part, I think the success of the continuous kiln lies in the proper manipulation of the draft, which is a matter which can be calculated according to the laws of physics.

Mr. Yates: What color goods does Mr. Lovejoy burn?

Mr. Cannan: Buff.

[The Chair at this point invited remarks from persons in the audience who were not members of the Society.]

Mr. W. A. Eudaly: I have found that there are very few things in connection with any kind of clay manufacture which are so attractive, and which siezes on us with such a terrific grip, as this idea of the continuous kiln. I never hear the subject discussed, that it does not take hold of me at once. Perhaps some of you don't know that I have spent the best part of my life in kiln manufacture, and kiln-study, and this thought invariably siezes on my attention.

I never have had the pleasure of going abroad, but I have no reason whatever to doubt but that abroad there are continuous kilns able to burn any kind of ware, because gentlemen say so, whom I know well. Yet, with all that, I don't believe we have ever had one in this country that would burn all kinds of ware, notwithstanding the fact, that many of the very best kiln builders the world has known have come to this country and built continuous kilns. (At least, we have been told that they were such.)

We have continuous kilns in this country that burn reasonably well. I mean by that, they burn common building brick very well. I don't know of a continuous kiln in this country burning very high grade pottery or white ware. I don't know whether in the old country Haviland ware and ware like that is burned in continuous kilns.

The Chair: The Miessen ware is burned in continuous kilns.

Mr. Stover: Mr. Griffin has told me that Haviland has just got in operation a successful continuous kiln for burning his ware.

Mr. Eudaly: Last year in a debate on this subject in the brickmakers' association, Professor Orton said such ware was burned in the continuous kiln, with which statement I then took issue. I did not know it had been done. It has not been done in this country; here, they burn just common ware. Regarding this kiln at Union Furnace, it may be possible that they are now burning any of their brick promiscuously in that kiln, but if so, it is only recently.

Professor Orton: My impression is that they are using the continuous kiln for classes of work which they did not use to do.

Mr. Eudaly: Possibly; Mr. Lovejoy is a very progressive man, and I would not be surprised if he had made progress in that line. I know some time ago he did not attempt to burn any delicate shades in that continuous kiln.

Let me call attention to one thing: most all these discussions concerning continuous kilns, are made with this idea in possession of us. We are enamored with the idea of the continuous kiln, and we discuss it from that standpoint. We discuss it half the time, as if it was practical; we say what we say, with an "if", "but" and "and", and believe it is coming to pass; but we ought to stick closer to the actual proven facts.

Can we get the heat distributed? Not a doubt of it, but there is one serious difficulty to be overcome; sometime, someone will overcome it. In burning face brick in a continuous kiln, driving the fire round and round, as it begins to heat, it will lean to this fire and cause the brick to roll a little on the face. That is the great trouble with burning in a continuous kiln, to say nothing of chasing the fire round and round, to get the brick to stand and settle absolutely straight, and be uniform, straight and come out as they went in.

Mr. Voelcker: There is no building made of face brick in Germany that were not burnt in a continuous kiln. We have no trouble there in keeping brick straight and true. The relation between the flues and the draft, and the setting, are the main things in burning in a continuous kiln. You must set face brick right, and give them the right draft. See that it is set all right, and don't pile them up fourteen or fifteen feet high and don't make the kiln too wide. The main part is to take your draft out above; if so, you have good results. As long as you take out the draft on the bottom you will get no good results.

Mr. Yates: I am sorry to trespass again, but there is one point I want to mention, in regard to people coming over here from the countries over there. I came here twenty years ago. Many things which they were doing in England successfully, failed here, when adopting the same process, same principle and method. I started to burning here without grate-bars, having been educated in Staffordshire where the potters never saw a bar. I found I hadn't the same coal and couldn't work it without bars. The conditions are different, and we must adapt ourselves to the conditions. I suggest that we study the down-draft kiln, which is not controlled by a current passing through horizontally, and I think we can utilize that principle in the continuous kiln.

Mr. Fiske: Speaking of Mr. Lovejoy's kiln, I don't think he would have any objections to my giving the figures in regard to its dimensions, which will go to illustrate the point which I stated here this morning. His kiln is a chamber kiln, divided by a permanent partition (illustrating on blackboard); these walls (indicating) make the chambers, which are in width about sixteen feet. The ware is put in here (indicating) to be burned; the fuel is put in through holes in the forward side of the chamber, and drops only in the fire-box. The air comes through here (indicating) and passing through the fire-box, passes up in this direction (indicating). It is something similar to the down-draft kiln, except his fire is on one side only. He uses the ordinary run of mine coal, I think.

Mr. Eudaly : Mr. Lovejoy had, and I think yet has, a supplementary furnace, which he fires from the outside.

Mr. Fiske: He has a wicket here (indicating) into which he shoves a few sticks of wood to dry out the ware a little, preliminary to putting the kiln gases in. Now, the point I am getting at is this. If we compare this with a downdraft kiln, we have got a kiln which is practically the equivalent of a down draft kiln thirty-two feet wide. The exit for the gases is at that point in the center; attempting to burn with the fire on the side, the fire must pass in that direction to the opposite side, 16 feet distant, In a down draft kiln we have fires on both sides, meeting in the center. Of course, we appreciate the fact that a down draft kiln 32 feet wide would be a total failure, because the goods in front of the fire would be burnt to vitrification, and the goods in the center would not be burned. That is the principal trouble Mr. Lovejoy has had in his kiln. I think he has put in a permanent checker work in the bottom and rear portion, to take the place of the brick which he found were invariably soft at the end of a burn.

If I understand Mr. Voelcker correctly, if we could cut that chamber right in two, making it eight feet instead of sixteen feet wide, we would have an immense improvement over what we have today. That is the reason I have taken the time to draw these diagrams, to explain what was brought up this morning. To be successful, the continuous kiln must be built narrow; and that is practically to what success is due in the European continuous kiln today. As far as I have observed, in my travels, there is no continuous kiln in America built less than ten feet wide. Every one which is not more than ten feet wide is successful for burning common brick.

Professor Orton: Mr. Fiske's statement is too broad, I think, when he says that a down-draft kiln, 32 feet wide, would necessarily be a failure. I know of some down-draft kilns 34 feet wide, which have given satisfaction.

Mr. Fiske: I was referring to a square kiln; of course, in a round kiln they may be even 34 feet. I don't remember the height of Mr. Lovejoy's kiln; it is very high. The most successful continuous kiln burning today in America is ten feet wide by eight feet high.

The Chair: Are the German kilns for finer wares smaller?

Mr. Voelcker: Yes, sir. Our best German continuous kiln is seven feet wide and six feet high. That is the best kiln in the world. For my part, in the brick business I find the Sieman & Rusk up-draft continuous kiln is the best. That is what everybody has got to look out for; the best for his particular purpose. The Sieman kiln takes all the smoke and steam out, drier and hotter. In other continuous kilns, the steam goes off at forty degrees centigrade.

The continuous kiln will burn good ware, if the man who handles that kiln, knows his work. It is generally not the kiln that is to blame. Sometimes it depends on the kiln. But if the continuous kiln was fed with pulverized coal with an automatic feeder, as Mr. Fiske's plans propose, I would expect a good deal from that kiln. The other matter suggested by Mr. Yates, the down-draft continuous system, I don't know anything about that. I know that when you take the draft off above, the steam can't condense to water on the bricks and you can't get whitewash. It is impossible. Down-draft, you might get whitewash.

Mr. Fiske : I think we would like to hear from Mr. Eisenhart, if we have time.

Mr. Eisenhart : I am not a member of this society. I am a brickmaker and don't feel that I ought to speak before this association. I don't know anything about fine brick, or fine pottery, but I have a continuous kiln in operation. I am satisfied from what experience I have had in burning common building brick, that one mistake in most continuous kilns is in having the chambers too large, and especially too high. As far as burning my ware is concerned, it don't make so much difference, as my kiln fires from the side almost exclusively. We don't burn any coal from the top holes until the bottom is red hot; then, when we throw a little coal, a good handful, in at the top, the minute it strikes the bottom it is gas; the finer the coal, the better the results. From what experience I have had with my kiln, I am satisfied that I could build a new kiln on that line, in which I could burn successfully face brick.

The Relation Between Kiln Atmosphere and the Color of Porcelain.

Mr. Griffin : I have asked to have this question put before the society, because I wanted to know more about it, not because I had anything to say. I would like some other member to tell me why it is that in burning porcelain it sometimes burns with a yellowish tinge and sometimes with a grayish tinge, the same ware. I don't pretend to know, myself.

Dr. William H. Zimmer : In regard to the influence of the kiln gases on porcelain, I would like to treat, first, of hard spar-porcelain. A kiln atmosphere can be reducing or oxidizing. The coloring matter in a porcelain body is the oxide of iron. The oxide of iron can be present as a ferric oxide, or as a ferrous compound, a silicate, which has rather a bluish white color. With ferric oxide, we get a yellowish tint. If we have a reducing atmosphere, we have all shades of white,—bluish white, grayish white, and a gray, which can actually be transformed even to a black by excessive reduction. This darkest color is due to carbon. When you get particles of carbon imbedded in your glaze, of course the black color that follows has nothing to do with iron, and is not to be referred to the iron.

If we come to the soft porcelains which bear a lead glaze, the influence of kiln atmosphere will be a little more complicated, inasmuch as the sulphur in the kiln atmosphere will have a more decided effect than on a leadless glaze. Of course, the coloring influence of oxide of iron will be just the same; you will have still the yellowish tint going over to white. But if your flame is too strongly reducing, it may reduce the lead in your glaze to little globules of metallic lead, producing a gray or blackish coloration.

Mr. Griffin : Then, do I understand that the whole thing resolves itself into the change of iron from the ferric to the ferrous condition, this change assisted by the moisture which would be in the kiln?

Dr. Zimmer: Well, moisture might have some influence; it might assist it. Moisture especially has a great deal to do with influencing color if you have sulphur in the kiln gases.

The Chair: Doesn't moisture intensify any action there, whether reducing or oxidizing?

Dr. Zimmer: Yes.

The Chair: Few men have fired white ware, who have not come across blued ware accidentally when they don't want it. The first thing to learn in the remedy of a disease is to learn the cause of it. If you can produce your disease, you can cure it. Has the question been answered to your satisfaction, Mr. Griffin?

Mr. Griffin: Yes, sir.

Should Our Society Adopt Standard Processes For the Testing of Clays?

Mr. Charles A. Bloomfield: I would like to speak from the point of a man who has clay to sell. I have been selling clay for twenty-four years. While I have found in a majority of cases the man who buys clay is thoroughly honest, I occasionally find a man whose superintendent is a thief. A man deriving his income from another, who will not give a sample a fair test, unless he is paid for it, and who will not give an order for supplies unless he is bribed, is a thief. And more than once I have come across a man who had his own ideas about testing, which same were very far from mine or of any one else.

There ought to be, to protect the buyer and the seller, some standard method of testing clays, as to their fire-resisting qualities. Of course, there can be no absolute method for testing clays for every purpose. But, as to the fire-resisting qualities, there should be a standard made and adopted by this society, which would protect every man here who is using clay, as well as he who is trying to sell it.

The Chair: I think it well to say that our committee on testing clays was organized to proceed on those very lines. There has been so much preliminary work to do, that very little has been accomplished in formulation of the actual tests themselves. But still, we don't propose to let the matter drop. I am chairman of that committee, but unless more rapid progress be made, I recommend that the society presently discharge me from that position, and put some one else in.

The work of the committee so far has been directed towards discussing what tests are necessary. We began with the analysis, but that goes but a short way. There are a great many considerations to be dealt with, and so far the committee has not completed the experimental work to show what tests shall be necessary, to say nothing of producing the tests themselves. That work of standardizing the tests themselves is a preliminary work, which has got to be done.

Mr. Bloomfield: Mr. Randall publishes a little book which he sells for one dollar, which contains the analyses of all the principal clays on the market. Regarding what you say about analyses, they are so similar to each other, that except for chemical purposes, one might say they are the same. Yet we know the clays themselves differ greatly, regardless of this similarity of composition.

Mr. Edward C. Stover: That point was brought out in Dr. Zimmer's paper. We get a chemical analysis of one clay which is much like another, but its action in practical work is very different, and the explanations in Dr. Zimmer's paper will account for some of these differences. I asked to have this question put on our list for discussion and when a man asks for information on a subject, Professor Orton at once puts him down for leading the discussion. I think a standard should be set for clay, so it will be understood by at least the men of the American Ceramic Society; not only a chemical analysis, but a rational analysis, and any other information that might be necessary to give as complete a description of the capabilities of the clay as possible; and I had hoped the testing committee might have a report to make in that line at this meeting.

Note on a Crystalized Kaolinite.

Professor Edward Orton, Jr.: The plasticity of clay is generally believed to be a function of quite a number of different properties. One of these properties is the strong basal cleavage which kaolinite crystals exhibit. Another is the immensely fine state of division in which they exist; another is the greasy feel which they possess, smooth and soft. These are some of the properties which are considered as influencing plasticity. The theory of the basal cleavage of the kaolinite crystals in flat parallel planes, explains plasticity on the view that these plane surfaces will slide readily on each other, in lines parallel to their planes, but will not pull apart readily in lines at right angles to these planes. Grains roll on each other without forming large contact surfaces, while plates slide on each other freely yet are in contact over such large surfaces that they have the cohesive attraction which will not allow them to be pulled apart. Such is the theory.

A number of years ago, Dr. Bischoff published a description of crystalized kaolinite, and gave in his article, illustrations of some of the shapes which the crystals assumed. He said that this sample was from the National Bell Mine of Denver, Colorado; I have been accustomed to quote that article each year to my students, showing them the crystal outlines, and explain that kaolinite theoretically assumes these beautiful and regular forms.

Mr. Samuel Geijsbeek, one of our esteemed charter members, has recently gone west, and established a pottery in Golden, Colorado. In the course of his investigations, when he first went out to that country, he met most of the men who were reputed to know something about the clays of that locality. He met one man who stated to him accidentally that he had years before sent some kaolin over to Germany.

Mr. Geijsbeek at once became interested and his friend, on thinking further, said he thought he might find the rest of the original sample somewhere about his office. The kaolin, he explained, had come from out of a small cavity in the rock in his mine. Mr. Geijsbeek persevered with his questions until his friend finally searched the package out of an old desk, where it had lain forgotten for many years. The paper in which it lay was covered with dust, but inside it was all clean. It was the other half of the sample which Dr. Bischoff had worked upon. Therefore it has a historical and sentimental value. In this bottle (indicating) is part of it. You will see that it has a crystaline glint, and does not look like kaolin. Ordinarily, kaolin is a perfectly amorphous solid, and gives us no idea of crystalization. Ninety-nine hundredths of this sample is composed of perfect crystals.

I have brought a microscope with me and on its stage I have put a little of this crystaline kaolin dust. I want each member of the society to pass in review before the microscope and see some real crystals of kaolinite. The sight is as rare as it is beautiful. The basal cleavage theory receives a considerable support from this sample, from the fact that where a mass of kaolinite of any thickness is on the glass slide, it is easy by wetting it and squeezing it with another glass plate, at the same time sliding the glasses one on the other gently, to cause the thick mass of kaolinite plates to slide out on each other like a new deck of cards. One plate overlaps another, in succession, until what was a little pile of compactly placed plates has been spread out in a long train of overlapping scales. This takes place only when water or some fluid is there, to act as a lubricating agent. I have such an overlapping train of scales in the field of the microscope now, which I hope you will all note.

My assistant, Mr. Peppel, has just completed an analysis of this sample, which he did not know was a part of Bischoff's sample, in which he finds the following proportions:

Alumina	46.40
Silica............	39.74
Water	14.06

[Thereupon a short recess was taken to allow the members to examine the sample under the microscope.]

What Relative Economy Exists in Burning White-Ware With Bituminous Coal vs. Anthracite Coal.

Mr. Thomas Gray : If some inventive genius could produce a kiln in which either hard or soft coal could be burned without making a change for each kind, it would be a great benefit to potters who during the last year have seen the price of these two fuels fluctuate.

Although I have just repaired my kilns and fitted them for anthracite coal, I am rather in favor of the bituminous coal, for a difference of $10.00 or $15.00 per kiln looks large. I have not studied into this subject very closely and am unable to give any data, except that my kiln-man tells me that the loss of saggars in my bituminous kiln is very much larger than in the anthracite kilns.

In 1893 I fixed my biscuit kiln for bituminous burning and my glost for anthracite. The biscuit kiln for the last four years, has required constant patching, while the glost kilns have needed very little attention. Soon after repairing the biscuit, I gave up using it, and it stood nearly a year without firing, using one of the glost kilns for biscuit. When I repaired all the kilns in 1900, the two anthracite kilns seemed to be in the best condition, although I put about the same repairs on all. My foreman claims that taking the loss of saggars and the repairs into consideration, that burning bituminous coal is expensive, even though the fuel costs less per kiln.

I have been told by a Trenton kiln-builder that the Trenton potters were convinced that the anthracite coal was the fuel to use. I cannot say how that is, but perhaps some of the Trenton people who are here can.

I have used a Nova Scotia coal—Dominion coal—but found it to be very expensive, although more than $1.00 per ton cheaper than Cumberland. It is a good gas coal, but heats too rapidly, and " kills " the saggars and we had to repair after each firing. The Boston and Philadelphia Face Brick Co. used it, and claimed they could save three to five tons per kiln. I have fired with oil, and this Nova Scotia coal has the same effect on the kiln as oil; the saggars crumble away very soon, seeming to lose their vitality, particularly those that came in direct contact with the oil or Nova Scotia coal.

Situated where I am, a kiln burning either hard or soft coal would be a great convenience, not only to myself but others as well.

Mr. William Burgess: My experience has been just the reverse of Mr. Gray's. We are farther away from bituminous coal than the East Liverpool manufacturers, who use it entirely, but we can get it for a dollar less than anthracite. In firing biscuit kilns with anthracite, and glost kilns with bituminous coal, I find less loss of saggers, and less loss in the wear and tear of the kiln. That is my experience.

Mr. Everett Townsend: We burn about the same amount of bituminous coal as of anthracite coal to a kiln, and, after keeping track of loss of saggers for several years. I can say there isn't much difference in the loss; it runs about the same with us.

Mr. Burgess: I think the wear and tear of the kiln is more dependent on the firing than on the fuel. When we commenced to use bituminous coal, we didn't know how to handle it very well; but as we become accustomed to it, we use it much more cheaply, and find it fully as satisfactory.

The Chair: Is your kiln up-draft?

Mr. Gray: Yes.

Mr. Edward C. Stover: There are a great many kilns of the down-draft type in Trenton, being fired with soft coal. I don't know which way the tide of opinion is running among the potters now, but they have changed some kilns to down-draft, and are using soft coal.

Mr. James Moses: I use altogether hard coal, and have had little experience with any other. I always thought the risk was very great in changing from one to the other, unless the fireman was used to the soft coal. I would like to use soft coal, but the value of the ware inside the kiln is so great that I have always preferred to let other people experiment first.

The Chair: Mr. Moses evidently likes to use the cat to take his chestnuts out of the fire. I think the question could best be settled by finding how many kilns have been changed from soft to hard coal, and how many from hard to soft. My experience goes to show that kilns are frequently changed from hard to soft, but I don't remember of any changed from soft to hard.

Mr. Burgess: My experience is identical with that. Over at the Ceramic Art Company, they have just applied the soft coal process in their new kiln.

The Chair: I may lay claim to having some hand in that: I started that before I left there. I think you will find the reason that you find both fuels used side by side is that those in business who are successful think they will let well enough alone and not risk a change. The potter is a conservative person and rightly so.

Mr. Gray: At Baltimore, they use soft coal altogether.

The Chair: They never use hard coal in England; they use soft coal altogether.

IN MEMORIAM.

WILLIAM GOLDING.

It is with deep regret that we are so early in our history, called upon to chronicle the removal, by death, of one of our esteemed members. On April 11th of last year it was the will of Him, who doeth all things wisely and well, to call from the activities of this life

MR. WM. GOLDING, M. E., of Trenton, N. J.

Mr. Golding was born in Forest of Dean, England, on June 13th, 1846. He came to this country with his father while still quite a boy. At an early age he was placed to learn the trade of a machinist. At the age of 21 he entered the Polytechnic College of Philadelphia, Pa., from which institution, in the year 1871, he received the degree of M. E.

After graduation he entered the employ of the Pennsylvania Steel Company, of Steelton, Pa. His ability soon procured for him advancement to the position of Manager of the "Bessemer" department. In 1881 he resigned from this position to take charge of the Trenton branch of the Golding & Co.'s Flint & Spar Works.

When, in 1885, the corporation of Golding & Sons Co. was formed, Mr. Wm. Golding was made its Treasurer. On the death of his father, in 1898, causing a vacancy in the office of President Mr. Golding was unanimously chosen to fill this office, which position he was occupying at the time of his death.

Mr. Golding was held in high esteem by the community in which he lived. He was looked up to as a business man of uprightness and integrity ; a worthy citizen and a prominent and active member of the Methodist Episcopal Church.

In the death of Mr. Golding we feel that we have lost one who would have been an able supporter of this Association and one whom we all could soon have learned to call by that endearing name of "Friend."

WM. BURGESS,
ERNEST MAYER,
Committee.

[The above was adopted by a silent rising vote.]

Remarks of President Binns on the Occasion of the Elevation of the First Associates to Full Membership.

Gentlemen:—We have this morning a pleasing duty to perform, and, in executing it, I would like to call your attention to the fact that this society consisted at its organization only of charter members, who bound themselves together in mutual co-operation for the advancement of the cause of ceramic science. At that time, expansion was fully intended; but it was laid down in what you might call, perhaps, a tacit agreement, or unwritten law, that those who should be admitted to the ranks of membership in the future should win their spurs.

We have always welcomed associate members, but it was the general feeling, of which I think you all recognize the truth, that mere numbers do not mean strength; and, while we have taken the position of welcoming to associate membership practically all those who ask to join, in order that they may receive and in time give, yet it was and is the feeling that the policy and management of the society should be vested in full members only and that membership itself should be restricted to those who gain that position by merit, and by merit alone.

Two conditions are imposed. The first one is that of technical ability; we require that a man who aspires to place his name on the roll of membership, shall prove by some specific contribution on the floor of the society this fact of his ability to sustain himself there. The second requirement is that technical ability shall be supplemented by a desire to impart of his stores of knowledge and to serve his neighbor and mankind. In other words, that altruistic spirit for which the society stands shall be kept to the front. It is not enough to know; any one may do that. It is necessary to appreciate as well the beauty of giving. Unless a man is willing to contribute a portion of his knowledge to his fellow members, here is scarcely the place where he will find his most congenial associations. It is well understood that many of us are the guardians of business secrets not our own, which we may not impart. But the proportion of our knowledge which the exigencies of trade and competition still require us to maintain in secret is small, and it is perfectly compatible with the interests of ourselves and our employers to impart knowledge on many lines of the greatest mutual benefit. No man can give without receiving; and we believe that no man should receive without giving according to his power and capacity to give.

The society has received applications from three associates for change of grade. This is the first time this has occurred, and I am sure that the members all welcome it with all the earnestness of which we are capable. The three associates whose applications for membership have been received are Walter M. Fickes, William Cannan, Jr., and Adolph F. Hottinger. Each of these men have presented contributions on the floor of the society. You have heard yesterday from Mr. Hottinger, whose contribution represents an enormous amount of work upon really useful lines. Only those who have been through such investigations know what work was involved in the few minutes talk Mr. Hottinger gave us yesterday in his brief abstract.

Mr. Fickes has presented to us, in lieu of the paper which he was prevented from delivering by the unfortunate accident which happened to him, his thesis which he presented for graduation in the Ohio State University. A resume of that was given to you by Professor Orton, and we in the council who examined it very carefully, came to the conclusion that it was work of a very high order, and the man capable of doing such work and imparting it, is the man we want.

Mr. Cannan you heard yourselves yesterday, and a similar remark will apply to him. His work is likewise of a high order, and we appreciate and value his candor and frankness in showing the results of his arduous labors.

I would like to say one thing in reference to two of these papers, for the purpose of encouraging those who may be willing to present ideas. Last year I presented a paper upon mono-silicate glazes, which was crude in itself and unsatisfactory to me but in it was the germ of an idea. To that paper can be attributed the work of Mr. Cannan and Mr. Fickes. Here is encouragement for those who are willing to do only a little work; they may plant a germ which may grow to maturity in the mind of another man. We never know in this line "How great a matter a little fire kindleth."

The Council has decided to recommend for full membership these gentlemen, and herewith presents their names to this society.

Mr. Bloomfield: Mr. President; I move that Mr. William Cannan, Jr., be admitted to full membership in the American Ceramic Society.

(Motion seconded and unanimously adopted.)

Mr. Stover: I move that Mr. Walter Morgan Fickes be admitted to full membership.

[Seconded by Professor Wheeler; unanimously adopted.]

Mr. Gates: I take the greatest pleasure in moving that Mr. Adolph F. Hottinger be placed upon the list of full members.

[Seconded by Mr. Griffin; unanimously adopted.]

President: Will the Council please come forward? (Messrs. Zimmer, Orton, Mayer, Burt and Wheeler assemble in semi-circle in the rear of the President.)

[By direction of the President, Messrs. Stover and Bloomfield conducted Mr. Cannan in front of the Chair; Messrs. Lovejoy and Townsend conducted Mr. Campbell (selected to act as proxy for Mr. Fickes in these exercises) to same position; and Mr. Hottinger was escorted by Messrs. Gates and Moses.]

President Binns: Gentlemen, the Council and the Amercan Ceramic Society, recognizing the merit of your work, and the enthusiasm which you have displayed in the cause of science, through its duly elected and accredited officers hereby declare you to be fully elected members.

We extend to each and all of you the right hand of fellowship, hoping that the ability you have severally shown in your chosen field of labor, and the devotion you have exhibited to the common cause may prove to be but a beginning.

To some it has fallen to turn the hard and crusted soil and to cast in a handful of seed; but it is upon you, the men of youth and fire, that the burden of the fight must eventually fall. You will meet with discouragement, disappointment and perhaps failure; but soonor or later you will hear the rustle of the growing grain, and it may be that you will even catch the golden gleam which heralds an abundant harvest. It will come. You are working for the future, and such works demands an unusual measure of self-sacrifice. Be not cast down if the ripening seems to tarry, but learn with patience to labor and to wait.

For myself, I account it a peculiar honor that this event has occurred on the occasion of my occupancy of the President's chair; and I feel a warm personal gratification that while director of the second ceramic school in the United States, I should have the pleasure of welcoming as members a graduate and a student of the first school. Gentlemen, we welcome you.

Thereupon, the new members received the congratulations of the president and members of the council, and the continued applause of the Society. After a brief interval the business of the Society was resumed.